THE TELEGRAPHIC JOURNAL AND

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COMPOUND WOUND DYNAMOS.

IT is now generally known that the specification to which we referred in our last issue as anticipating the claims put forward by the owners of the Brush patent is that of Mr. S. A. Varley. In our Correspondence columns will be found two letters bearing upon this matter, both of which demand comment. Mr. Swinburne lays great stress on the fact that neither in the specification of Brush nor in that of Varley is there any mention of winding for constant electromotive force. This is true, but the compound coiling is there nevertheless, and it appears to us that the proportions of shunt to series coils is one of degree rather than of kind. It must also be remembered that when Varley took out his patent we had no incandescent lamps to destroy by overrunning, yet he says, "Those parts of my invention by which the electricity which produces the light is developed are applicable for nearly all purposes where electricity in large quantities is required." Turning for a moment to the Brush specification with its *sixteen lines* of provisional and *thirteen pages* of final matter, we assume that the vital portion of the invention is clearly stated in the claims. Claim 8 runs thus:—"A dynamo-electric machine wherein a portion of the current produced or capable of being produced thereby is diverted for the purpose of maintaining a permanent magnetic field, substantially as shown."

Varley's says:—"Part of the electricity developed by the machine is diverted to maintain the magnetism of the soft iron magnets."

Brush's Claim 9 runs:—"In a dynamo-electric machine the wire or helix, E, having a comparatively high resistance and kept in closed circuit while the machine is running, in combination with the magnet wire or helix, F, as commonly employed substantially as shown."

Varley writes:—"I prefer to wrap the soft iron magnets with two insulated wires, one having a larger resistance than the other—the circuit of larger resistance is always closed, and the circuit of less resistance is used for the electric light."

We do not agree with Mr. Sellon in his arguments as set forth in the other letter referred to above, that "the description referred to has *nothing whatever to do* with the point at issue," for if so neither has the matter contained in the Brush specification; and, as to one's knowledge of the most elementary principles of cause and effect, the fact that Mr. Varley patented and constructed the first reaction dynamo before Wheatstone and Siemens communicated their papers on the subject to the Royal Society, that he anticipated Brush in double coiling, and that he also eliminated certain coils on his armature, as does Brush, at certain parts of its revolution, will all be sufficient to show that his reputation, at all events, is in no danger of being shaken by the remarks contained in our correspondent's communication.

The above paragraphs appear to us, even without securing the aid of an electrician to make matters more clear, so convincing that no further comment is necessary, and we shall be curious to see in the face of what has been published in our pages, whether the Anglo-American Corporation will still try to enforce its advertised menaces. We may add that the machine constructed by Mr. Varley in 1876 is still to be seen.

THE BRITISH ASSOCIATION.

(Specially Reported for the "Electrical Review.")

ON A NEW FORM OF GALVANOMETER.

By Prof. JAMES BLYTH, M.A., F.R.S.E.

(Section A.—Mathematical and Physical Science, September 16th.)

THE object of this short communication is to describe a method of measuring electric currents by means of an instrument whose scale is graduated to equal divisions, and whose deflections are, from the intrinsic nature of the instrument, proportional to the current strength through all ranges.

In principle the instrument depends upon the mechanical action exerted by a magnetic field upon a movable conductor carrying a current. In construction it is essentially similar to the well-known apparatus, due to Faraday, for showing the continuous rotation across the lines of magnetic force of a horizontal radial conductor carrying a current and having slipping contacts at its centre and circumference. The construction will be understood from the following description:—

Two bundles of permanently magnetised steel wires are made up in the form of cylinders having narrow axial holes. These are fixed with their axes in the same vertical line, separated by a narrow gap, and so that the north poles of the one set of magnets face the south poles of the other. In this gap the magnetic field is sensibly uniform and the lines of force vertical. Inserted in the gap is a thin circular disc of wood or vulcanite, having a central mercury cup and a concentric circular mercury trough at a short distance from it. A stout brass rod is provided, having a short thick copper wire rigidly attached at right angles to its lower end. Its upper end passes freely through the axial hole in the upper magnet, and is rigidly attached to the lower end of a long fine torsion wire of steel or silver, whose upper end is fixed to a suitable support. The lower end of the brass rod dips into the central mercury cup, and the outer end of the copper wire (bent down a little at right angles) dips into the concentric canal of mercury. Stout copper wires are led from the central cup and canal to binding screws suitably placed so as to form the terminals of the instrument. The upper end of the brass rod, which projects a little above the upper magnet, carries a long pointer, which moves over a horizontal circular disc, graduated either to degrees or to show ampères directly. This disc is fixed with the torsion wire passing freely through its centre. The whole is so enclosed as to be free from air current.

The action of the instrument will be easily understood. Suppose a current sent through it. As is well known, the electro-magnetic force acting on the radial current will tend to make it rotate in a horizontal plane with a uniform force. This rotation will go on till a certain angle is reached, when the moment due to the electro-magnetic force is balanced by the moment due to the torsion of the wire.

Let

i = the current strength,
a = the length of the radial wire,
N = the magnetic induction,
A = the torsion constant,
θ = the angle of equilibrium,

we have

$$\tfrac{1}{2}\, i\, a^2\, \mathrm{N} = \mathrm{A}\,\theta\,;\ \text{or}\ i = \frac{2\,\mathrm{A}}{a^2\,\mathrm{N}}\theta,$$

showing that the current strength is proportional to the angle of deflection.

The instrument admits of several modifications. I have constructed two in addition to the above. In the one the permanent magnets are replaced by similarly placed electro-magnetic coils, through which the current to be measured passes. In this case the square of the current strength is proportional to the angle of deflection. In the other the fixed magnets are dispensed with, and, instead, a cylindrical bundle of magnets is fixed coaxially to the brass rod, but insulated from it, so that it rotates along with the rod and radial wire. In this case the radial wire projects from the middle of the length of the magnet bundle, while the lower end of the brass rod is prolonged so as to dip into a mercury cup at a considerable distance below the lower end of the bundle. Owing to the influence of the exterior part of the circuit leading from the mercury canal to the cup, the equation for this case is a little more complicated. With proper arrangements, however, the deflection can be shown to be proportional to the current strength.

In the instruments constructed for practical use the mercury cup and canal are so made, on the unspillable ink-bottle principle, as to preserve the mercury in case of being knocked over or even inverted.

MOLECULAR DISTANCES IN GALVANIC POLARISATION.

By J. LARMOR, St. John's College, Cambridge.

(Section A.—Mathematical and Physical Science, September 10th.)

IT has been shown, principally by Helmholtz, that polarisation involves a condensing action on the surface of the metallic electrodes. The particles, say, of the cathion which exis in

the electrolyte in a state of temporary dissociation are drawn towards the cathode by the electromotive force till further approach is prevented by chemical forces. Thus along the surface of the cathode we obtain a sheet of positively charged cathion molecules, which are in equidistant arrangement on account of their mutual repulsion; and opposite to each, on the material of the electrode, there is the equal and opposite charge drawn there by electrostatic induction. This double sheet is equivalent to a condenser.

Kohlrausch and Helmholtz measured the charge required to produce a given polarisation difference of potential, and therefore estimated the thickness of this double layer.

Lippmann (*Comptes Rendus*, 1882) determined the surface energy of the chargè per unit area from the variation of the capillary constant of a mercury electrode, and thereby gained an estimate in agreement with the above.

We should expect that the capacity of the double layer per unit surface would remain constant until the distance between contiguous cathion particles became of the same order as the thickness of the double layer, *i.e.*, until the contiguous particles began to feel one another's chemical forces and be influenced thereby. An examination of Lippmann's results shows this constancy of capacity to a very close degree for a range of about a volt. We may accordingly conclude that with his acidulated water polarised to a volt the particles of the polarisation layer have become so numerous as to be in chemical contact with one another. This leads to a third estimate of a molecular distance, which agrees with the two former.

The first estimate is based on the absolute electrostatic measurement of the charge; the second on the measurement of a surface tension; the third on the absolute electro-chemical equivalent of the electrolyte. The complete agreement of three estimates founded on physical constants so various and of so different orders of magnitude is strong evidence of the validity of this method of interpreting the phenomena of polarisation.

They are all in satisfactory agreement with the estimates of Sir Wm. Thomson and others from different considerations (*Nature*, 1877, or Appendix to Thomson and Tait's "Natural Philosophy," Part II.), and give an average result of about one 10^{-10} metre, more or less.

DISCUSSION.

LORD RAYLEIGH thought this a most interesting subject, but he should want to give more attention to it to follow and appreciate Professor Larmor's reasoning. It was certainly a remarkable effect that he obtained, taking data coming from such various quarters. The whole was of the greatest importance, and wanted all the light which could be thrown upon it. He (Lord Rayleigh) had, however, hardly followed it sufficiently to give a criticism on Professor Larmor's results.

The PRESIDENT said there were great difficulties in experimenting in this direction. Some time ago he was seeking for some such end as that which Professor Larmor seemed to have arrived at, but by quite a different way. He was trying to form estimates. In the experiment he tried he had the advantage of the use of a very good compression pump and a polarised plate of hydrogen; and he tried to submit the latter to a pressure of between two and three tons to the square inch. The result was to destroy the polarisation altogether. He produced this result several times. His idea at first was that he should polarise the plate by immersion. In that he failed.

Prof. OLIVER LODGE said Helmholtz regarded water as a kind of double condenser, no strain at all existing in the interior. Following out his hypothesis, it was easy to calculate the surface density and the difference of potential needed to tear asunder oppositely charged electrodes attracting each other across molecular distance x. Let it be σ, and let the charge of an atom be q; then

$$2\pi\sigma q = \frac{q^2}{x^2}.$$

19,320 electro-magnetic units can decompose 18 grammes of water, which allows $536 \times 3 \times 10^{10}$ electrostatic units of each kind of electricity per gramme; so if n^3 be the number of molecules in a gramme of water the charge of each dyad atom is

$$q = \frac{1{\cdot}6 \times 10^{13}}{n^3}; \simeq 1{\cdot}6 \times 10^{-11} \text{ probably.}$$

Hence

$$\sigma = \frac{1{\cdot}6 \times 10^{13}}{2\pi n^3 x^2}; \simeq 3 \times 10^{-12} \text{ probably;}$$

and, the difference between either plate and liquid being given by $4\pi x\sigma$, the whole difference of potential between the electrodes is

$$v = 8\pi x\sigma = 6{\cdot}4 \times \frac{10^{13}}{n^3 x}; \simeq {\cdot}0064 \text{ electrostatic units probably,}$$

or 1·92 volt. In reckoning the approximate numerical values above, he had assumed that $n = 10^8$ and that $nx = 1$. The resulting decomposition force came out, quite accidentally as far as he was concerned, very near that needed to decompose acid water. But he need hardly point out that order of magnitude was all that was really calculated in these figures; and that not only decimals, but even numerals, prefixed to the power of 10, had very little meaning. The value of x might perhaps vary somewhat for different substances; and as to the value of n, though that was definite enough, it was very probable that it ought to be something more like 2×10^8 than what he had assumed. It was, at any rate, satisfactory that the electrostatic hypothesis should give, as a polarisation E.M.F., a number so entirely comparable with actual results.

REPORT ON ELECTRICAL THEORIES.

By Prof. J. J. THOMSON.

(*Section A.—Mathematical and Physical Science, September 11th.*)

I HAVE confined myself in this report exclusively to the consideration of those theories which only profess to give mathematical expressions for the forces due to a distribution of currents, and I have not considered those theories which profess to give mechanical explanation of those forces. I had not time to consider both classes of theories, and it is evident that the mathematical theory must be settled before we can arrive at a satisfactory mechanical one.

As the general result of the enquiry, we may say that all that has been proved is that it is absolutely necessary to take into account the currents in the dielectric, and that the action of these, as well as other currents, must be given by some form of the potential theory, that is, the theory started by F. E. Neumann and generalised by V. Helmholtz. But nothing definite is known as to what we should take as the measure of these electric currents, and which of the many forms of the potential theory is the right one.

We hardly require experimental proof that alterations in the polarisation of the dielectric, at any rate if the dielectric be other than the other (?), produce effects analogous to those produced by an ordinary current flowing through a conductor, for the polarisation of a dielectric by an electromotive force involves a change in the structure of the dielectric; this is shown by the alteration in volume experienced by glass and other bodies when placed in the electric field, and also by the breaking down of the dielectric when the strength of the field is great enough. Now if we move a magnet we shall, since we produce an electromotive force in its neighbourhood, produce a change in the structure of the dielectric around it, because we alter its state of polarisation. It follows, then, from the principle of action and reaction, that if we alter the state of polarisation of the dielectric we shall alter the state of motion of the magnet, so that an alteration in the polarisation of the dielectric produces a magnetic force; we can show in a similar way that an alteration in the polarisation must produce all the effects produced by an ordinary conduction current. We know nothing, however, about the magnitude of the current, which is equivalent to a charge in the state of polarisation; it seems natural to suppose that the intensity of the current is proportional to the rate of charge of the electromotive force. Let us suppose that it equals

η (rate of charge of its electromotive force).

The quantity, η, has never been experimentally determined, but two hypothesis have been made as to its value by Maxwell and Helmholtz. According to Maxwell $\eta = \kappa/4\pi$ when κ is the specific inductive capacity, and according to Helmholtz η is also a fraction of κ.

There is very little experimental evidence for either of these theories; for Maxwell's theory, perhaps the best evidence is, that if we assume the electro-magnetic theory of light, the refraction index should, if $\eta = \kappa/4\pi$, equal the square root of the specific inductive capacity, which is very approximately the case for a good many substances. Maxwell's assumption has the great advantage of getting rid of all discontinuity in the currents, and when this is the case all forms of the potential theory lead to the same result, so that if we could prove Maxwell's theory experimentally it would be a complete theory of electro-dynamic action. If it should turn out, however, that Maxwell's theory is not true, then we should have to go on further and determine which of the several forms of the potential theory is the true one, as if the currents are not closed the different forms of the theory lead to different results.

It would seem that the most important thing to be done in electro-dynamic theory is to determine whether $\eta = \kappa/4\pi$ or not, and I have described two ways in which this might be done. If Maxwell's theory should prove not to be true, then we must go on to determine the value of η for all dielectrics, and which of the forms of the potential theory is the true one.

ON CONSTANT GRAVITATIONAL INSTRUMENTS FOR MEASURING ELECTRIC CURRENTS AND POTENTIALS.

By Sir WILLIAM THOMSON.

(*Section A.—Mathematical and Physical Science, September 11th.*)

THESE instruments, the author stated, were parts of two series of electrical measuring instruments for current and potential which he was now working out. In the two current instruments—the milli-ampèremeter and the hecto-ampèremeter—the mode of effecting the measurement was founded on Faraday's law, that a ferro magnetic mass placed in a variable magnetic-field experiences forces tending to make it move from places of weaker to places of stronger force. The essential parts of the milli-ampèremeter are shown in the sketch diagram, fig. 1. It consists of an electro-magnetic coil, shaped as shown, fixed with its axis vertical, and a little bar of soft iron hung from one end of a light balanced lever so as to be free to move up and down in a circular arc, deviating but little in its middle and at its two ends from the axis of the coil.

The measurement is obtained from the deflections indicated on a scale by the end of the balanced lever when a weight of known amount is hung on the ring below the iron mass. To screen the iron from the effects of the earth's magnetism the coil is enclosed in an iron box.

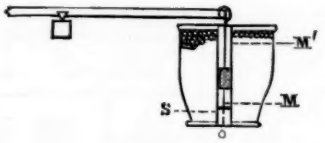


FIG. 1.

In the hecto-ampèremeter the variable magnetic-field is obtained by a suitable disposition of the metallic conductor conveying the current to be measured. The conductor may be taken as consisting of two thick copper plates, shaped according to the sketch, fig. 2, supported parallel to each other, say one centimetre apart and metallically connected at the place indicated by A. At

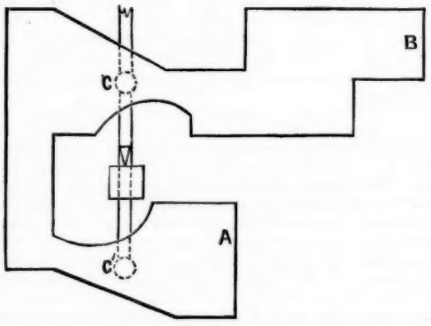


FIG. 2.

B is fixed a suitable electrode. The course of the current is therefore from B to A, and from A through the other plate to the part of it corresponding to B, which forms the other electrode. In this way two similarly varying magnetic-fields are produced, and the balanced lever carries two masses of iron, one in each field. In other respects the instrument is similar to the milli-ampèremeter.

The electrometer consists of an air condenser with one of its plates capable of a to and fro motion so as to vary the capacity of the condenser.

The fixed brass plates are supported so as to be accurately parallel to each other and in metallic connection, while they are thoroughly insulated from the case of the instrument. The movable plate is of aluminium, and is supported in a vertical position on a knife edge; the plane of its motion being parallel to the fixed plates and situated midway between them. The upper end of this movable plate has a fine prolongation which serves as a pointer for indicating the deflections on the scale of the instrument, and at its lower end is fixed a knife edge having its length perpendicular to the plane in which the plate moves.

When the fixed and movable plates are connected respectively to two points of an electric circuit, between which there exists a difference of potential, the movable plate tends to move so as to augment the electrostatic capacity of the instrument, and the magnitude of the force concerned in any case is proportional to the square of the difference of potential by which it is produced. In the use of the instrument this force of attraction is balanced by the horizontal component of a weight of any convenient amount hung on the knife edge at the bottom of the movable plate.

ON A METHOD OF MULTIPLYING POTENTIAL FROM A HUNDRED TO SEVERAL THOUSAND VOLTS.

By Sir WILLIAM THOMSON.

(*Section A.—Mathematical and Physical Science, September 11th.*)

THE method described by the author was to arrange the necessary number of condensers in series. A terminal is connected to each plate, and the series of terminals is conveniently placed so that by a suitable mechanism a pair of movable terminals, between which a known difference of potential exists, may be brought successively into contact with each adjacent pair of unfixed terminals, moving always in the same direction along them.

DISCUSSION.

The PRESIDENT said he had listened with the greatest interest to Sir Wm. Thomson's descriptions. No doubt others had been struck with the same thing which had struck him, the curious theoretical property on which the instrument was founded. An approximately spherical body was the last thing for measuring the magnetic permeability of a substance, and it was very curious that that awkwardness should have been put to such admirable use in this instrument.

Dr. W. H. STONE: As Sir Wm. Thomson has mentioned my name, I would ask him which of these instruments is most suitable to my researches. I am using an E.M.F. of only from 4 to 8 volts, and through a resistance approximately of 1,000.

Sir WM. THOMSON: An ordinary astatic galvanometer would be best for you.

Prof. FITZGERALD remarked that there seemed to be an advantage sometimes in suspending on two sharp points instead of on a knife edge. It was a very much simpler method, and obviated the danger which existed of dust getting on to the knife edge. He would also like to know whether Sir William had made any experiments as to the temperature curve of coils by introducing into the middle of the coil a length of carbon filament, because the temperature curve for carbon was in the opposite direction to that for ordinary metallic wire.

Prof. EWING scarcely understood what Sir William said as to the reversal of the current as affecting the residual quantities. He could easily understand how a reversed current, when attended by a diminution of the current, would account for any removal of the residium; but he could not understand how this could be the case with a constant current. He did not think much would result from the mere reversal of a constant current, and if any diminution of the residual maximum was caused by the tapping described by Sir William Thomson, he should ascribe it more to the disturbance of the instrument.

Prof. FORBES said he might answer the question of Prof. Fitzgerald with respect to carbon. He had made experiments, the general result of which was that it was perfectly easy to make a compensating resistance which should give the same resistance in places of different temperatures; but when used with the potential galvanometer, in which the current you were using itself heated the carbon and the metal, the relative heatings were not proportional, and therefore it was not possible to use carbon as a compensator.

Sir WM. THOMSON said with reference to the temperature curve of coils, it was quite impossible to correct by having carbons part of the circuit. He aimed at arranging resistance coils which would be exceedingly little heated. He did this by winding the wire on an open bobbin with an open core—cores far larger than usual in resistance coils—so that in the first instance there would be a large surface both inside and outside for carrying away the heat. He did not allow an outer sheet. He thought a very thin coat of ribbon would probably do very little to keep in heat. A sheath of vulcanite, or such material, is sometimes used to cover resistance coils. A copper cylinder would be a disadvantage, for it would at a little distance keep off too much heat. He absolutely objected to that. Besides that, he arranged in cases of special experiments to put a block-tin tube through this and pass a current of water through it, so as to keep the temperature of the metal almost absolutely constant. He had tried the method of suspension suggested by Prof. Fitzgerald, but never got any results; it always broke down, and he had gone back upon the orthodox balance method; nothing else was fit for fine work. He had special arrangements for bringing this back to its position. With regard to Prof. Ewing's remark as to tapping, he rather under-estimated the fact that the magneto effect is due to reversal. He had repeated the tapping on several occasions, and it did shake out the residual magnetism if there was any to be shaken out.

The PRESIDENT thanked Sir Wm. Thomson for his exhibition of his beautiful instruments, and for his very lucid and interesting description of them.

ON MEASUREMENTS OF THE INTENSITY OF THE HORIZONTAL COMPONENT OF THE EARTH'S MAGNETIC FIELD: MADE IN THE PHYSICAL LABORATORY OF THE UNIVERSITY OF GLASGOW.

By THOMAS GRAY, B.Sc., F.R.S.E.

THE general principle of the method adopted was that of Gauss, with these modifications. In the *deflection experiment* two deflectors were used simultaneously. These were used one on each side of the magnetometer needle, with their axes in a line at right angles to the magnetic meridian; and in position for deflecting the needle the direction of the line passing through the centres of the deflector and the magnetometer needle were, *first*, the magnetic meridian, and second, a line at right angles to it. The author pointed out that in the equations resulting from this arrangement the length of the deflector entered as a minus term in the one, and plus in the other, thus rendering the observations highly sensitive to possible errors in that term, and enabling a fairly good estimate of the effective length of the different deflectors to be had. Apparatus was described by which the whole cycle of observations, both for this and the *oscillation experiment*, might be obtained without the necessity of handling the deflector magnets.

The effect of the various corrections to be made on the value of H derived from the equations is discussed, and such as were sensible were made the subject of direct experiment, and their effect allowed for in the table of values of H given below.

Date. 1885.	Number of deflector.	Approx. dia. of deflector in cms.	Length of deflector in cms. (dia. = ·25).	Weight of deflector in grammes.	Distance of centre of deflector from the magnetometer needle in cms.		Distance of the scale from the magnetometer needle.	Effective length of the deflector.	Magnetic moment per grm. of the deflector.	Horizontal intensity in C.G.S.	Mean of each set of results.
					East and West positions.	North and South positions.					
May 27	1	0·25	8·03	3·050	32·06	28·75	108·7	6·91	44·9	·1520	
...	2	„	8·05	3·063	...	...	„	7·10	58·5	·1524	·1522
„ 28	3	„	8·05	3·075	...	...	„	6·31	54·1	·1521	
„ 29	4	„	8·05	3·067	...	...	„	7·11	52·3	·1522	
June 5	5	„	4·00	1·526	30·0	25·125	„	3·12	35·2	·1524	
...	6	„	3·99	1·522	...	...	„	3·72 (?)	33·7	·1524	·1525
...	7	„	4·00	1·525	...	...	„	2·33	31·7	·1527	
„ 10	8	„	14·933	5·646	51·9	38·85	„	13·22	55·8	·1527	
„ 11	9	„	15·030	5·727	...	...	„	12·82	64·3	·1525	·1526
„ 11	10	„	15·021	5·666	...	...	„	13·58	54·5	·1527	
August 21	10	0·20	10·01	2·318	35	30	128·9	9·14	71·0	·1526	
...	11	„	10·01	2·336	...	...	„	9·14	62·7		
„ 26	10	„	10·01	2·318	...	...	„	8·98	71·0	·1527	·1526*
...	11	„	10·01	2·336	...	...	„	8·98	62·7		
„ 31	12	„	10·00	2·318	...	...	„	9·14	70·0	·1526	
...	13	„	10·005	2·336	...	...	„	9·14	61·8		

* Corrected to noon for diurnal variation.

DISCUSSION.

The PRESIDENT remarked that this was no doubt a very technical matter, but one, as experts knew, of very great importance.

Lord RAYLEIGH said his experiment was on the attraction of the two coils through which the current went, and he had chosen that method because it was more accurate than anything involving the use of the measurement of h. But he was bound to say that Kohlrausch, who had given great attention to the subject, and discussed it very elaborately, came to the conclusion that methods of this character did lead to very accurate knowledge of horizontal magnetism, and on their authority one might take it as established. But he confessed that his preference was very much for a method that did not involve the measurement of h, particularly as he had to work in a laboratory where other electrical determinations were going on, producing disturbance. Probably Kohlrausch's methods involved the use of a suitable locality, where disturbance was effectually guarded against. A convenient use of the method required an auxiliary instrument by which the fluctuations of the magnetism following from moment to moment might be corrected by a small instrument. Otherwise it was difficult to say, during a lengthy process of this kind, whether the determinations really corresponded.

Mr. WHIPPLE observed that Lord Rayleigh had spoken on the very subject on which he was going to ask a question—respecting the time required to make a determination of h by this method. He had worked for many years on the Gauss instrument, and he knew that they could not make a determination of that under much less than an hour. During that hour, independently of the regular change which is always going on, due to what was called diurnal change and other things, there were frequent occasions when the changes of force were very considerable, and those changes of force entirely disarranged the observations made by this Gauss method. In fact, it was a practice with him to discard the observations, and do nothing at all with them, if he found there was any important change going on during the hour, and thus, in the course of a year, many important observations were lost. Therefore it was important to get some instrument to show this in a shorter time. He gathered that this lengthy process was, in fact, only a modification of a process which had been in use many years. What was wanted in the way of improvement of the existing apparatus was the production of something more compact and portable than the existing apparatus, and also cheaper. He was constantly applied to for information as to the obtaining of some cheap and handy apparatus for travellers to measure the magnetic force of different parts of the earth's surface, and there was nothing he could recommend. He thought the question of the relative value of deflections made end on and broadside on, had been worked out by Sir George Airey in his "Treatise on Magnetism." He (Sir George Airey) also gave accounts of the methods used for determining the introduction of temperature corrections, which were also necessary for the employment of the magnet satisfactorily.

The PRESIDENT suggested that Lord Rayleigh, Mr. Gray, and Mr. Whipple should turn their resources to account in investigating a new method suggested lately, which had been practised more or less by Helmholtz and Topler, and which was said to have some advantages in the way of abbreviating the time during which they were subject to variations in the earth's force. When methods of measuring the h were discussed, this was a thing that should not be lost sight of. With regard to the question of the advantages of the end on and broadside on methods, he did not think, as far as he recollected, although it was ten years since he read the treatise, that Sir George Airey carried the mathematical calculation far enough to settle the point in question.

Sir WILLIAM THOMSON said the deflected magnet was infinitely smaller, practically speaking, so small that there was no dimensional error in it at all comparable with the sensibility of the apparatus. The end-on method was preferred to the broadside method on account of the greater deflection it gave, but the great merit of using both, as had been pointed out by the author, was that dimensional errors tended to increase the result in the one method and diminish it in the other. With respect to errors due to possible change in the magnetic moment of the deflectors, this could be guarded against by systematic observations of the deflections they give on the magnetometer. The best form of magnetometer magnet was two very thin discs of glass-hard steel placed a short distance apart.

The PRESIDENT, in expressing the thanks of the section to Mr. Gray for his paper, said it called the attention of practical men to the important fact that the dimensions of the deflector magnet used were small. In this respect the method differed from the continental procedure, and even from the practice in this country, where the Gauss pattern of magnetometer was used.

A vote of thanks was then accorded to the author of the paper.

THE HENNEBERG-LORENZ ARC LAMP.

BEFORE entering upon the description of our lamp, we will make a few remarks on Davy's arc light as it guided us in the construction of a new arc lamp.

If we establish, by means of a constant electromotive force the electric arc between two carbon points which are at a short distance from one another, and then leave it to itself without any further regulating of the carbons, the number of ampères, which was at the outset very great, will in accordance with Ohm's law decrease, whilst the volts will increase, as in consequence of the carbons burning away the length of the arc, and with it its resistance, increases. When at last the electromotive force is no longer able to produce a current, the length of the arc will not be capable of further increase and the light will be extinguished.

This case will also afford an illustration of the optical variations of the arc. The original reddish light, principally due to the mere incandescence of the carbon, passes as the arc increases in length gradually from yellow to white in consequence of the development of the blue rays which are the complement of the yellow rays. When the arc still further increases in length the blue and the violet rays predominate, whilst at the same time a vivid flame makes its appearance, which soon becomes unsteady, begins to rotate round its axis, and is extinguished.

We mention this for the purpose of pointing out that alterations of the length of the arc are not only accompanied by fluctuations of the current, but, also, necessarily by fluctuations of the light. This way of

observing the arc affords a good practical means of determining the proper length of the arc for given carbons and for a given electromotive force, and to arrange the lamp accordingly. It will also be seen that the same kind of carbons burn away more rapidly under the influence of a larger number of ampères than in the case of a smaller number being used. The rapidity of the burning away of the carbons is consequently a function of the length of the arc.

If we now take into consideration that those phenomena occur under the usual relations between current and carbons, within a length of arc of from 4 to 5 mm., we must admit at once that the arc light is very sensitive to alterations of the length, and that a great number of complicated causes and actions (such as fluctuations of the ampères, of the volts, of the intensity of the light, and of its colour, and also of the rapidity with which the carbons burn away) arise from an alteration of the length of the arc amounting to only 1 mm.

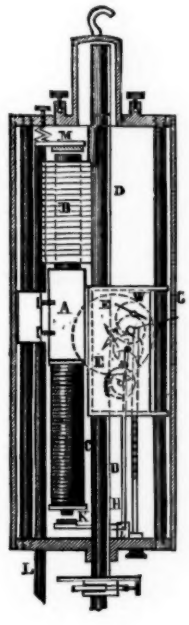
But the state of affairs presents a totally different aspect if we maintain unaltered the distance between the carbons requisite for a certain length of arc. As the consumption of the carbon is very steady, the movement of the carbons must not cease for an instant; it must not be intermittent, but it must be effected carefully and steadily throughout. It must not only be steady, but neither too slow nor too rapid, and always proportional to the consumption of the carbons, as otherwise the phenomena attending the alteration of the resistance of the arc would exert a disturbing effect. If, then, the electromotive force is perfectly constant, and if the carbons are homogeneous, their movement must be uniform, but if the electromotive force, and with it the burning away of the carbons, varies, or if the rapidity with which the carbons burn away is not constant on account of their not being of uniform structure, the regulation must be more rapid or slower, as the case may be. If those conditions are carefully fulfilled the light will be steady and uniform, the ammeter and the voltmeter will not fluctuate, and an upright rod will cast a shadow well defined, absolutely immovable and of constant intensity.

From the foregoing remarks it is evident what our definition of an electric arc light regulator is. The question is not merely to reduce the distance between the carbons, when they have burned away to some extent; but it is *to move the carbons towards one another at a rate proportional to their consumption*, that is, as under this condition the ampères and the volts are constants *to effect this whilst the current remains constant.* Practically such an arc light regulator must fulfil the following conditions: *the apparatus must ensure a steady movement of the carbons, neither intermittent nor totally interrupted by periods of rest, and must, moreover, be capable of effecting that movement with variable rapidity.*

We do not believe that an arc lamp has yet been constructed upon those principles.

The question is then to apply a mechanism that will impart to the carbon holder a movement not accelerated by the gravitation of the earth, but uniform and uninterrupted. This mechanism consists of a train of wheel work provided with a fan and actuated by the weight of a rack, which also acts as carbon holder. In this way the condition of a continuous and uniform motion is fulfilled, but not that of independence of the current. In order to fulfil the latter condition an iron disc is adapted to some part of the wheel work, which rotates freely between the pole pieces of an electro-magnet magnetised by the current. In this case advantage has been taken of the physical fact that an iron disc rotating between the poles of a magnet is impeded in its motion by those poles. This phenomenon we had, of course, to study more thoroughly before proceeding to the construction of our lamp, and we found that with a constant load of the rack each current-strength corresponds with a certain number of rotations of the disc. When, therefore, a current of given strength is sent through the coils of the electro-magnet the disc will rotate with a constant corresponding velocity; if another, say a weaker, current is sent the disc will continue to rotate with a constant, but in this case greater velocity. In this way it is possible, in the first place, to effect the movement of the carbon whilst the current remains unaltered, and, in the second place, to attain variable velocities for currents of variable strength to overcome irregularities in the carbons and in the electromotive force, and also to work with currents of variable strength. The rapidity of the motion of the carbons in our lamps is therefore—the disc being actuated by the weight of one of the carbon holders—a function of the strength of the current.

The construction of the lamp is explained in the diagram. A represents the pole pieces between which the disc, E, rotates, B is the principal electro-magnet and C the shunt electro-magnet. The rack, D, actuates by means of the train of wheel work, F, the disc, E, which rotates freely between the pole pieces, and also the fan, W. L is the lighting apparatus which is attracted at M, by which the arc is formed. H is a lever which serves to prevent the rack, D, from sliding downward when the lamp is not in action. This is effected by the pawl, G, which drops into a ratchet wheel on the axis of the disc.



The action of the lamp is now as follows: let us assume that the carbon points are not, when the lamp is not in action, in contact with one another, then the whole current will circulate through the shunt electro-magnet, C. This electro-magnet will consequently be enabled to attract its armature, K, which, being pivotted at I, moves the lever, H, thus releasing the ratchet wheel at G. The disc, E, will now rotate and at the same time the rack, D, will slide down until the carbon points are in contact with one another. The principal circuit is now closed and the electro-magnet, B, magnetised. This magnet attracts, on the one hand, the armature, M, thus forming the arc, and, on the other hand, it magnetises the pole pieces, A, by which the rotation of the disc, E, and with it the movement of the carbon is stopped. This condition continues until the arc has attained the requisite length. Whilst the arc increases in length its resistance likewise increases, the result of which circumstance is that more current passes through the coils of the shunt electro-magnet, C. But the magnetism which C induces in A is the opposite of that which B induces in A, therefore the more the arc increases in length the weaker becomes the magnetism of the pole pieces, and the disc begins to be released slowly at first and then more rapidly. However, at the moment when the rotation of the disc is exactly proportional to the consumption of the carbon, no further

alteration can occur in the system neither in the length and the resistance of the arc and in the strength of the current, nor in the magnetism of the pole pieces; and the movement of the carbons is therefore steady and uniform.

The essential difference of our system from other systems lies, then, in the circumstance that the lamps can be regulated without alterations of the current. The lamp is also theoretically independent of the so-called "sensitiveness of the regulation" for this only operates at the commencement of the regulation, whether the disc begins to rotâte somewhat earlier or later. Other systems, in which the movement of the carbons is unsteady, subject the lamps to a test of the limit of the alteration of the current on which the mechanism still will act. It is also clear that these lamps, in consequence of the interruption of the movement of the carbon holder, are themselves the cause of the alterations of the current, which are again balanced by the carbons nearing one another. Notwithstanding the independence of our lamp—the electromotive force being constant—of the sensitiveness, the use of the rotating disc allows of far greater sensitiveness than can be the case with any mechanical arrangement.

In conclusion, say the inventors in *Centralblatt für Electrotechnik*, we point out that the lamp as constructed by us acts in reality as described and the steadiness and constancy of the movement of the carbon is a matter of fact.

SOME THOUGHTS ABOUT THE TELEPHONE.

THE discovery of the microphone by Prof. Hughes, and the construction of metal microphones capable of transmitting speech, have altered the aspect of the question affecting the originality of the ordinary telephonic transmitters in use. It is now seen that the crude transmitting apparatus of Reis, however imperfect in a practical sense for the purpose of speaking to a distance, does of necessity contain the practical principle of the microphone, although Reis himself may not have been entirely aware of it. He did not discover the microphone pure and simple, operating without a tympan, and as a general property of loose contacts between conductors traversed by an electric current; but he hit upon a form of microphone combined with a tympan, which acted as a current regulator, and was capable of transmitting spoken words or parts of words. The discovery of the true microphone remains with Prof. Hughes, in spite of what both Reis and Edison did, because he elucidated the whole subject, and the conditions of microphonic action, besides inventing apparatus which operated in a different manner, *i.e.*, without the use of a tympan and under the direct action of the air, or through molecular vibration of a body as distinct from tympanic pressure. He also showed how, by his delicate microphonic contacts, these vibrations could be rendered audible, and sounds, as it were, magnified. But the discovery of even an imperfect microphone in combination with a tympan by Reis, and designed for the purpose of transmitting sounds and speech, has an important bearing on the claims of Edison for originality in the "carbon transmitter," which is virtually a microphone in combination with a tympan. If the practical principle was contained in the apparatus of Reis, it follows that Edison is not the first and true inventor of a microphonic transmitter combined with a tympan, and that his original invention is limited to details of construction, the use of plumbago, &c. It also follows that other inventors are free to use the principle in question, with modifications of detail introduced by them, provided such details are not those invented and protected by Edison.

If the practical principle of an invention is public property, it stands to reason that it is only in the method of carrying it out that any subsequent improvement can be claimed as a new invention.

Let us consider this point at greater length. The Edison patent, which holds the ground against so many microphonic transmitters, claims "the combination of a diaphragm or tympan with *a tension regulator*, substantially as described." In some recent forms of transmitter there is no tympan in the ordinary sense; but the point may be disputed in a court of law. It therefore appears that if the Edison patent be held to include all kinds of tension regulators, whether of carbon or metal, in combination with a diaphragm, it must also include the platinum transmitter of Reis, which can be regarded as a platinum microphone combined with a tympan. It is true that the combination claimed by Edison is specified to be "in a closed circuit," and it is usually believed that Reis's instrument interrupted the circuit. But this is only true in part. One of Reis's instruments (date 1860–61), as a look at it will show, had a spring pressing on the contact as if to keep it close, and it could also be worked without breaking the circuit, as, perhaps, it did. The fact that Reis obtained words through it shows that it acted sometimes as a microphone, or true tension regulator, in the Edisonian sense. It matters not that Reis himself did not fully understand its working, and spoke of interruptions. The instrument was there, and we can now understand better than he how it works. I have heard words transmitted by the platinum contacts of a transmitter which was simply a copy of Reis's instrument. It follows that Reis's transmitter, being a microphone when the sounds were not too strong to interrupt its circuit, antedates the Edison patent, and invalidates its claim. This is a view of the case which might legitimately be advanced on fair grounds, and which on due consideration must recommend itself to an electrical expert, whatever he may think of it at first.

One can admit that Reis did not *publish* the microphonic principle as Prof. Hughes did, and that he probably did not realise its existence, although it must to a certain extent have operated in his instrument, judging by his results, and the actual microphonic construction of the apparatus. But Edison himself does not appear to have realised or published the microphonic action at work in his original carbon transmitter. On the contrary, he seems to have regarded its operation as due to an inherent property that plumbago possessed of varying its resistance under pressure. It was Hughes who discovered the peculiar microphonic principle of a loose or imperfect contact, and this fact ought never to be forgotten. It was only after Hughes's discovery (which applied to metals as well as carbon, conductors as well as semi-conductors), that Edison's instrument was seen and shown to work on the microphonic principle; and the Blake instrument, if not also the carbon button instrument, were fashioned by the light of Hughes's discovery.

It follows that if we allow Edison's instrument to be a form of microphone, we must also allow Reis's 1860–61 instrument to be another form of microphone. Both of them consist of tympans in combination with tension regulators. The use of carbon in one, and metal in the other, has nothing to do with the question, for since Prof. Hughes's discovery, and the construction of actual speaking instruments with metal instead of carbon contacts, it is known that metals also make tension regulators. The weakest point in the argument is that Reis's instrument did not always work in a closed circuit; but this is a question of degree rather than of kind. With louder sounds the circuit would be interrupted, with feebler sounds it would remain "closed" in the sense that the current would not be quite broken, though "regulated" in strength. The circuit of a "Blake" transmitter could also be broken if one spoke loud enough. Every microphonic arrangement is liable to interruptions of the circuit by loud sounds; and the spring in the Blake instrument which presses the contact surfaces together has its exact counterpart in the spring which Reis employed to press his platinum surfaces in contact. In fact, the Reis 1860–61 transmitter is essentially a microphone like the Blake. It has all the elements of the Blake

instrument, tympan, contact points, and spring adjustment. Substitute hard carbon for one of the contacts in the Reis instrument, and you have substantially a Blake transmitter.

The reason why the Reis contacts are so apt to open the circuit, as compared with the contacts of the Blake, is that they are both of platinum. Had one been of carbon this difficulty would have disappeared, and M. Reis would have thoroughly succeeded in producing a practical telephone. He would, in fact, have completely anticipated both Edison and Bell. As it was, he failed from the materials he used, rather than in principle. I have found from experiment that metal contacts, and especially platinum ones, are very apt to open the circuit under the vibrations of the voice, and the reason probably is that the "arc" or air discharge which connects them when they are vibrated is very short; shorter than in the case of carbon, if not some other metals. Hence, in order to preserve the continuity of the circuit, the vibrations of the voice must be much feebler than in the case of carbon. This was the chief difficulty in the way of Reis; a difficulty of detail, not of principle. We have metal contacts now which overcome it, without having to resort to carbon at all.

The chief points of the argument, then, as set forth above, are as follows :—

1. In 1860–61 Phillip Reis constructed a telephonic transmitter with platinum contacts, and a receiver consisting of an electro-magnet with a thin iron armature in front of its poles, and vibrated by the current. (See Silvanus Thompson's work, entitled, "Phillip Reis: Inventor of the Telephone," for a full description of the apparatus.)

2. The Reis receiver is essentially a Bell receiver, the difference between them being one of detail.

3. The Reis transmitter is essentially a metal microphone; of an imperfect construction, it is true, but still a microphone. The metal used was platinum.

4. Edison's transmitter is essentially a carbon microphone. Blake's transmitter, which is a form of Edison's, is a carbon and platinum microphone.

5. Reis's microphone was combined with a tympan, so is Edison's and Blake's.

6. Being a microphone, Reis's transmitter is a "tension regulator" of essentially the same nature as Edison's and Blake's; the difference consisting mainly in the materials used, and being one of degree rather than of kind. Both can operate "in a closed circuit," provided the loudness of the voice is graduated to suit each instrument.

7. Therefore Edison's claim for *the combination of a tympan with a tension regulator in a closed circuit*, is anticipated by the Reis transmitter (and cannot be sustained).

The claim of Edison is, in fact, too comprehensive—fatally so. Had he confined his claim to the use of lamp-black, plumbago, or even semi-conducting materials (as he did originally in his English patent before it was amended to make it more sweeping), for his "tension regulator," it might have been more secure; because we do not know that Reis used such materials. But, in extending it, he has brought his claim into antagonism with the earlier work of Reis.

There is no need to dwell upon the work of Prof. Hughes, because it is his discovery which has thrown light upon the whole subject. Moreover, it was apparently Prof. Hughes who showed Edison himself, and likewise Blake, that hard carbon contacts were preferable to lamp-black and plumbago. It is only since the Hughes microphone was brought out that hard carbon has been used in transmitters with such success. Prof. Hughes has also enabled us to see that Reis's instrument was in reality a metal microphone, and led the way to the production of metal microphones which act as transmitters. These instruments might be regarded as improved forms of the Reis microphone. So may Edison's, since they are all "tension regulators" in a closed circuit. Moreover, they have this advantage, that they have no tympan like the Reis instrument, and are in this respect more distinct from it than Edison's instrument.

The disuse of a "tympan" also comes from Prof. Hughes's discovery, since he found that a tympan was unnecessary.

The question as to what constitutes a "tympan" has been much debated in this country, and some eminent authorities have held that anything constitutes a "tympan." Some are believed to have said that the earth itself constitutes a "tympan." For example, if you lay a microphone on the ground, the latter becomes a tympan. Did Edison, then, intend to patent the earth? Surely not. But even if he did he could not. The supposition is absurd, but it serves to show the fallacy of the view. What these authorities meant was that the earth would act as a physical vibrator. But here we discern how the fallacy arose, for a vibrator in the physical sense is not necessarily a "tympan" in the technical sense, any more than a block of wood is a table, or a bundle of straw is a folding bedstead. By a "tympan or diaphragm," Edison meant a tympan or diaphragm in the usual sense, and nothing else. Had Professor Hughes not shown that these well-known devices were not necessary to the working of the microphone, we should never have heard that a tympan was anything else but what it is generally understood to be; and it would not have been necessary to strain the meaning of the word so as to make it include such diverse objects as a block of wood, a quantity of water, or of any vibratory body.

Indeed, if any vibratory body is to be considered as a tympan, we cannot exclude the material parts of the microphone. Hence the Edison claim may be interpreted as the combination of a tension regulator with itself—a conclusion which is absurd.—J. MUNRO, in *La Lumière Electrique.*

GAS ENGINES.

THE Committee of the Franklin Institute intended making a report upon the gas engines exhibited at the International Electrical Exhibition held last year in Philadelphia. It turned out, however, that after a conference with the exhibitors, the latter were unwilling to submit their engines to the tests proposed by the committee.

This is certainly unfortunate, as such information as that which might have been acquired would be invaluable. The proposed tests were as follows :—

CODE OF TESTS OF GAS ENGINES.

The aim of the experiments will be two-fold :—

I.—A PRACTICAL TEST to determine the efficiency of the engines exhibited under conditions regarded as the most favourable by their makers, with a view solely to the acquirement of practical information regarding the economy and reliability of the several engines.

Such determination will consist of :—

a. A ten-hour test under maximum load.

b. A ten-hour test under minimum load.

c. A ten-hour test under average load, the engine being stopped at the end of each hour, and the time lost in re-starting noted.

The data collected will consist of :—

1. Indicator cards.
2. Prony brake readings.
3. Velocity and regularity of rotation.
4. Total number of revolutions.
5. Total number of explosions.
6. Temperatures of entering gas and air.
7. Temperature of exhaust gas.
8. Temperature of entering water.
9. Temperature of escaping water.
10. Pressures of entering gas and air.
11. Analysis of entering gas.
12. Analysis of escaping gases.

II.—A SCIENTIFIC TEST to determine certain details of the action of the gas inside the cylinder, valuable and desirable in view of the imperfect state of our knowledge of gas engines from a theoretical standpoint.

For this purpose some one engine will be selected, and the attempt made to obtain the following data, in addition to those secured in the practical test :—

1. Temperature of gases after explosion; by means of the electrical resistance of fine platinum wire placed in the cylinder.
2. Effect of varying amounts of compression.
3. Effect of varying mixtures.
4. Effect of compressing the gas and using it through a reduction-of-pressure regulator.
5. Effect of heating the entering gas.
6. Inflammability of the charge by an electric spark at different points of the cylinder
7. Effects of variations of speed and changes of valve settings, &c.
8. Dissociation experiments.

THE GREENOCK ELECTRIC LIGHTING.

ON Thursday, 10th September, the following report by the Electric Lighting Committee to the Police Board was submitted and unanimously approved of :—

"We have to report that the contract for the supply and fitting up of the electric lighting plant at Prospect Hill, entered into with Mr. E. W. Beckingsale on 10th and 13th October, 1884, was completed on 9th March last, and certified by Professor Jamieson as being in perfect working order. The electrical apparatus has been working successfully since the last date, and on 12th May Professor Jamieson reported as follows :—

"College of Science and Arts, 38, Bath Street,
Glasgow, *May 12th*, 1885.

"To the Greenock Police Board Committee on Electric Lighting :—

"Gentlemen,—I have much pleasure in reporting that Mr. Beckingsale has carried out the electric lighting at Prospect Hill in full accordance with your specification and contract. I frequently visited the work during progress, thoroughly tested the whole system upon completion, and again only last Friday, after the lamps had been in action for over 500 hours, I tested the system at work. All my tests are duly recorded in the official test book at Prospect Hill. The high insulation resistance maintained in the underground cables and wires is due to great care in handling them, close superintendence during laying them, and a constant application of searching electrical tests. The dynamos and testing apparatus are all of the *best* make and suited for their work.

(Signed) "ANDREW JAMIESON, Principal.

"There are twenty street lamps regularly lighted in the area set apart under the Provisional Order; and three lamps in the machine house and grounds. Mr. Beckingsale, by his contract, undertook to maintain the whole electrical apparatus for a period of six months after completion, and this period expired to-day (9th September, 1885). The work has been satisfactorily upheld during this period, and the contract is now at an end.

"During the month of August last arrangements were made with the Roxburgh Street Refining Company, whose refinery is situated within the compulsory area, for a supply of electricity to light their char-house for a period of three years from date of commencement. The fitting up of the apparatus in the refinery was entrusted to Mr. Beckingsale, but the whole cost is payable by the Roxburgh Street Refining Company. The rate to be charged for each lamp by the Police Board is £1 per annum for the time during which the ordinary street lamps are lighted; and the number of lamps is about ninety, which are to be maintained by the Refining Company. The charge will be from 8th September, as the lighting was commenced on that date.

The total cost of the undertaking is as follows :—

1. Mr. Beckingsale's contract for apparatus, &c. ...	£873	9	2
Do. do. for maintenance ...	73	0	0
2. Cost of machine house, making trenches, &c. ...	386	7	6
3. Turbine pulleys and gearing	151	10	0
4. Consulting engineers	51	0	0
5. Expense of Provisional Order and printing ...	216	4	10
	£1,751	11	6

ORDINARY WORKING.

"The maintenance having expired, two men are engaged in attending the machinery, at a total cost of £2 10s. per week, which, it is expected, will be reduced during the summer months. The renewal of lamps will cost about 15s. per annum each.

"The revenue for the refinery contract, and from the public lighting, is expected to yield £133 per annum, which will almost meet the cost for wages and upkeep.—Reported by

"JN. DUFF, Convener.

"Greenock, *9th September*, 1885.

[Mr. Beckingsale is to be congratulated upon the successful termination of his labours, which were carried out under difficulties of no ordinary nature.—EDS. ELEC. REV.].

Electric Lighting in the Royal Navy.—The ironclad *Black Prince*, 28, Staff Commander Charles Brent, has made a most satisfactory trial of her electric lighting machinery at Devonport, when the contractors, Messrs. R. E. Crompton, of Chelmsford, were present.

THE ELECTRIC LIGHT AND ATMOSPHERIC ABSORPTION.*

The absorption of light by the earth's atmosphere is a subject of very great importance, for it affects and modifies our views of everything around us. We recognise its importance to ourselves when we reflect that seeing is our only means of direct communication with bodies at a distance. The sense of smell carries us a little way out of ourselves, and the sense of hearing carries us a little farther, the one through fine particles carried by the atmosphere, and the other by its regular vibrations; yet the direct effects of these are manifest only in the immediate neighbourhood in which they are produced, and it is only by means of some agency—such as electricity or some other mode of motion—that sounds can be transferred into some other form of energy and retransferred so as to reproduce themselves as signals at considerable distances. The sense of sight reveals not only the outline and colour of bodies immediately around us, but that very absorption by the atmosphere of which, as our president has told us, we know so little and of which I have to speak, enables us to judge of distance, and lends the charm of tone and colour to the landscape; it warns us of the gathering cloud, and in the twilight of the morning it tells us of the coming sunshine.

Light reveals to us not only things within our atmosphere, but Prof. Stokes, in his course of lectures here, has shown the various ways in which it may be employed to investigate the condition and composition of the sun and stars, and to resolve the nebulæ. Within the last ten days it has revealed to us that in the centre of a nebulous mass of matter there has been a concentration of the luminous gases into a central mass, as though a new sun were just caught in the act of its formation. Yet, we must remember that we have now only received the record of an event which happened many thousands of years ago, and that the waves which have brought it to us, travelling at the rate of 192,000 miles a second, have been on their journey for that long period of years. Since the days of Newton we have learned to regard the sun's light as composite in character. With a wedge or prism of glass he showed that it consists of different kinds of radiations, which are bent or refracted in different degrees on passing through the prism, and that the result is a spectrum of different colours—red, orange, yellow, green, blue, indigo, and violet, where each degree of bending is accompanied by a special shade or colour, and the sum of all combined makes up the total sunlight. The sun's heat has been studied independently, and has also been found to consist of radiations, or we may call them rays, which are bent in different degrees on passing through a prism. The same is true of the actinic power or chemical action of the sun, each degree of bending corresponding to a definite amount of chemical or photographic power. Not only so, but corresponding to a given degree of bending by the prism, or rather corresponding to a given wave length, the heat, the light, and the actinic power from the same source are always present in the same proportions. The same is true of the spectrum of the electric light from glowing carbon poles, or of the radiations from a glowing gas. Take, for instance, the green band of thallium. The burning of this substance at the temperature of the electric arc causes it to give out its own special radiation in the green, which possesses a certain amount of heating, light-giving, and actinic powers, and these are so related that whatever alters the amount of the one alters each of the others in the same proportion. If, for instance, the vapour of thallium at a lower temperature absorbs one-fourth of the light, it also absorbs one-fourth of the heat and one-fourth of the actinic power of this green ray. This statement is not inconsistent with the fact that there are rays of obscure heat, which give no light, and rays which are chemically active, but which give no light and very little heat, but will embrace these cases, only the proportion of light in them is nothing. Take again the rays of different wave length, such as we may regard radiant heat, light, and actinic power as only different results, or as different forms of the energy of the self-same radiation. Let us examine the spectrum of selenium. In each of these rays, then, the heating power, the light-giving and actinic power, are each present there in a definite form and always in the same relation to one another, but not in the same proportions for the different *bands* (colours). Any substance which absorbs a ray of definite wave length will have absorbed its heating, its light-giving, and its actinic powers in the same proportions, although we may not be able to detect the presence of them. And here comes our difficulty, the difficulty of measuring and comparing these effects, which are not absolute but relative, and which are always being dissipated, so diminishing our stock of useful energy. To get over this difficulty of the measurement of the energy of radiations, one takes his photographic camera, and, devoting himself to *fastening on* a special portion of the spectrum, he maps its energy by means of its action on certain prepared chemicals.

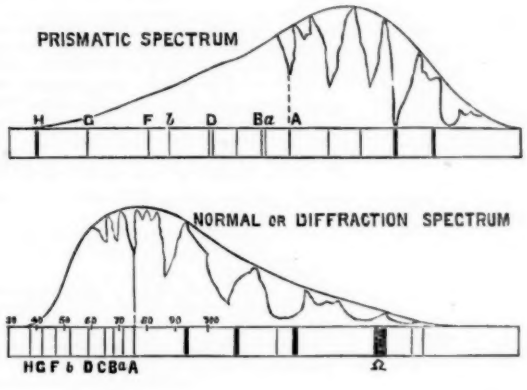
In this field there are many workers at the violet and ultra-violet rays, which produce strong chemical action on salts of silver, but for the red and beyond the red end of the spectrum the progress made is especially due to Captain Abney, F.R.S., who has greatly extended our knowledge in this direction, and opened up and made accurate chemical measurements in parts of the spectrum which were before unknown, to which the door had

* A lecture delivered before the British Association, at Aberdeen, on Friday, September 11th, 1885.

only just been thrust a little open by Herschel and other investigators, who detected differences of chemical action in different parts of the red end of the spectrum. Another observer takes Melloni's thermo-electric pile, which measures the energy of radiations by the change of temperature produced by them at the junctions of two metals, antimony and bismuth, the change of temperature being measured by the thermo-electric current so produced. [Experiment with thermopile and galvanometer.] Now to get nearer and nearer to the single radiations the breadth of the thermopile must be more and more reduced, and with it the electric current which deflects the needle of the galvanometer. With this instrument the general distribution of heat throughout the spectrum and beyond the red end have been measured. But even the line thermopile has considerable breadth, and so Prof. Langley invents his bolometer, wherein by the change of temperature of a wire of platinum from $\frac{1}{1000}$ to $\frac{1}{10000}$ of an inch in thickness, *i.e.*, so fine that it almost requires a microscope to see it, he measures the heating effect of solar and other radiations. With this instrument, which is far more sensitive than any linear thermopile, Prof. Langley has investigated the energy of the radiations throughout the solar spectrum. He has shown us that, although Captain Abney had extended the spectrum nearly twice the length of the visible spectrum beyond the red, and had made far more accurate measurements in this region than any which had been made before, yet he had not told us half the tale, for Prof. Langley's investigations show that the portion beyond the red is six times as long as the visible spectrum. Let me explain what I mean when I speak of the length of the spectrum. With prisms we obtain spectra of different lengths and unequal spreading of the colours, *i.e.*, unequal bending of waves of the same length, according as we change the material of our prisms. Hence, in measuring the heat or the chemical action in a length of, say, 1 mm. of the spectrum, we get one value with a glass prism, another with quartz, another with rock salt, and another with bisulphide of carbon in the same region of the spectrum, and with the same general bending of the waves we shall have a different amount of spreading out or dispersion of the waves of different lengths, and an unequal lengthening of the spectrum in its various parts. Not only so, but the long waves are compressed together, and the short waves are spread out, so that as regards wave-length our estimate of the heating and the energy of 1 mm. in the red, and beyond the red is far too high, because the waves are there too much compressed. In the spectrum of the sun and of the electric arc the greatest heating in a length of 1 mm. of the spectrum formed by a prism is beyond the red; but if we arrange the distribution of the colours of our spectrum according to wave-lengths, the greatest heating effect, or the maximum energy, lies in the yellow, *i.e.*, the part which gives the brightest light. We may get more exact ideas by comparing diagrams in which the length of the spectrum is represented as a horizontal line, and the intensity at any point of it is represented by a height drawn upwards from that point. The bounding line through all the upper ends will then enclose an area which will represent the (distribution of) energy throughout the spectrum. The prismatic spectrum is apt to give us wrong ideas, from the crowding together of the red end of the spectrum. Reckoning wave lengths in 10,000ths of 1 mm., the ultra-violet extends from about 4 to about 2½/10,000ths of 1 mm., the visible spectrum extends from 4 to about 7·6/10,000th, and in the infra red Captain Abney has measured to a wave-length of 12, and Prof. Langley has measured or shown the existence of waves whose lengths extend beyond 24/10,000ths of a mm., *i.e.*, nearly 1/10,000th of an inch. With the visible part, from wave-length 4 to 7·6/10,000ths, in which part also lies the region of maximum energy, we are principally concerned this evening. We have as yet no established photometric method for the absolute measure of visible radiations, but there is great promise that we are not far from as delicate a test for light as Prof. Langley's bolometer is for radiant heat. This delicate test depends upon the very remarkable sensitiveness to light which is possessed by the substance selenium. Specially prepared selenium has less electrical resistance on exposure to light than it has when kept in the dark, so that placing it in the dark and joining it up in an electric circuit with a battery and galvanometer there will be a greater current, causing a deflection of the galvanometer needle when the selenium is exposed to light.

All the visible rays of the spectrum change the electrical condition of selenium; but the greatest change is produced by the greenish-yellow waves—that is, the brightest part of the spectrum. The obscure heat rays beyond the red produce comparatively little change, and the ultra violet rays do not seem to have the power of producing any change in the selenium. Hence selenium gives a very delicate mode of measuring the illuminating power or the energy of the visible radiations by the increase in the electric current, as shown by the deflection of the galvanometer. Last year I received from Mr. Fritts, of New York, some selenium cells, similar to the one I have here, which were far more sensitive to light than anything which had been previously tested. This cell had an electrical resistance of 200,000 ohms in the dark; it was reduced to 940 ohms—that is, less than $\frac{1}{200}$th part of the original resistance by exposure to sunlight. I have found that the exposure of one candle at a distance of one foot reduces the resistance of this specimen to one-half of its previous value. Another cell had an electrical resistance reduced from 75,000 to 43,000 ohms by the same exposure. In this change of 32,000 ohms for the illumination of one candle, we see that we have the means of measuring very small differences of illuminating power, and therefore a means specially adapted to measure small differences due to absorption of light by the earth's atmosphere. A series of observations taken throughout the day with the selenium cell exposed to the south-eastern portion of a cloudless sky showed a gradual increase of current from half-past nine to half-past eleven in the morning, a maximum steady current lasting until about half-past one, followed by a gradual diminution of current from half-past one until four o'clock. It will be seen that those substances in which the energy of radiations in the visible spectrum seems to be directly converted into electrical energy gives promise of a means of determining the energy of simple radiations from the sun or the electric light or any other source, and also of determining the amount of absorption by solid, liquid, or gaseous transparent media.

I should like to show the effect produced upon this selenium by different portions of the spectrum of the electric light; and therefore I will expose it in different portions of that spectrum. It is connected now [experimenting] with a galvanometer and a battery of small power. I will make the connections. We will try to bring the spot of light to a point on a scale, and then expose this selenium to the light of first the blue and then the stronger light of greenish-yellow end of the spectrum. I now expose the selenium to the blue. You observe the spot of light moves towards my right. Now I transfer the selenium to the green part of the spectrum: there is a stronger action than before. Now I close it again; we come back. Now I expose it to the yellow, and you observe it goes nearly to the end of the scale. I remove this from the more refrangible toward the bright or greenish-yellow part of the spectrum: the deflection of the spot of light increases, showing that as we get towards the portion of highest illumination the effect increases.



When the beam of white light falls upon the coloured substance a portion of each kind of light is regularly reflected, giving a reflected beam of white light. Another portion of the light enters a little way into the substance, and after being reflected from molecule to molecule is thrown back again and scattered in all directions. This effect reveals the presence and colour of the substances. The remainder of the light enters and penetrates deeper into it and becomes absorbed, the energy of those radiations being converted into energy of motion of the molecules of the body. Or a portion may pass through; in that case its colour will be complementary to the colour of the light thrown back or scattered. This effect may be seen in the case of liquids, and especially liquids in which fine particles are floating, such for instance as the deep brown waters of a mountain stream; or if to water a little milk be added, then the difference in the transmitted and the reflected or scattered beams is at once apparent. But milk and water, being opaque, or partially opaque, scatters by reflection the light which falls upon it and appears blue, whereas the light passing through the liquid is a brown. Any mixture which is partially transparent and has very fine particles suspended in it will produce similar effects, and similar effects may be produced by opalescent glass. Resin dissolved in alcohol and mixed with water gives a blueish tinge seen by reflected light, and imparts a yellowish-red, or even red tinge to light which has been transmitted through it. An explanation of these effects is at once seen by examining the spectrum of light which has been reflected from the liquid. Those of us who come from the neighbourhood of London, or from large manufacturing towns, have abundant opportunity of studying the effect produced when sunlight is transmitted through various thicknesses of smoke full of very fine particles which have not been fully consumed. We may see the effects of this absorption of the light by passing light through resin dissolved in alcohol, as I have just explained. When the liquid is here we ought to get a beam of white light through it. [Experimenting.] I am afraid that here we have not absolutely clear, but still in certain portions of the field we have our white beam. There may be a little of the remains of previous experiments which takes away from the whiteness. But if now a little of this liquid be added, we shall see at once that the colour of the transmitted light will be browner, but there will be light scattered from this substance by reflection, which will be of a pale blue. Adding a little more adds to the brownness of the light trans-

mitted, and causes this liquid to be far more luminous. A little more and we see that effect still more increased, and the addition of still a little more will show that the intensity still more increases. The transmitted light is brown; the intensity of the light scattered by reflection from the particles within this liquid is increased.

The absorption by gases also is interesting, and may be studied by the same means. The experiments of Stokes, Bunsen, and Kirchoff on sodium vapour in front of the slit of the spectroscope will illustrate what takes place in such cases. If a source of light behind the slit sends a beam through it, and that beam passes through the sodium flame, the sodium flame will add to the brightness of the sodium band in the spectrum as long as the source of light behind it is weaker than itself. But if the source of light behind the slit is of stronger temperature than the sodium flame some of the vibrations will be stopped, and if there be sufficient difference of temperature in the two flames a black band will take the place of the bright sodium bands; and we have at once the explanation given by Bunsen and Kirchoff of the dark lines of sodium and other substances in the spectrum of the sun. The presence of the vapour of sodium and other substances in the upper regions of the sun's atmosphere causes the absorption of the bands peculiar to these substances, and the light from the lower regions of the solar atmosphere is deprived of those bands in passing through; and so it comes to us with dark lines in the position of those bands. When the molecules can vibrate at different rates the dark lines are sometimes grouped in one part of the spectrum and weakened by one colour more than another, as, for instance, with vapour of iodide. In a liquid state the molecules are too close to allow of their free motion. Their vibrations are disturbed, and the absorption of the bands widened out as the thickness of the medium is increased. The characteristic bands of substances and their changes may be studied in dilute solutions of liquids and in coloured glasses.

I will now show you the effect of this absorption with sodium vapour—a well known experiment, but one which very well illustrates this branch of the subject, and therefore I take it as representative of this class of experiment. The light from the electric arc now passes through the prism, and in front of the slit is this flame of gas into which a little sodium will be introduced. The temperature of that flame will be below the temperature of the electric arc, and therefore we may expect that the portion of the light corresponding to the position of the sodium band will be cut out from the light of the electric arc. The sodium band is in the yellow spectrum, and at present we see the yellow light; but I hope in a moment or two that the black band will appear in the position of that sodium band. [Experiments conducted; black band appearing after a short interval.]

Not only glowing vapours, but gases at ordinary temperatures produce black lines in a continuous spectrum. We may see the lines of absorption by nitrogen and oxygen in the atmosphere by sending a beam of light through nitrous oxide fumes giving many telluric black lines, many black lines due to the passing of light through the gases of the earth's atmosphere. In introducing this gas, we may see that there will be dark bands in the continuous spectrum of the electric arc. Here we have these dark bands cutting out particular portions of that spectrum, hence cutting off a portion of the blue end of that spectrum from the density of the gas through which the light passes. Besides these there are many other gaseous vapours in our atmosphere, and especially the vapour of water, each absorbing light of special wave lengths and adding to the dark telluric rays. But these vary greatly with the quantity of water vapour in the atmosphere, and the length of path through the atmosphere traversed by the beam. The group known as the sodium band, which I showed just now, is formed partly of metallic lines and partly of rays of absorption due to the vapour of water. These atmospheric lines, known as the rain bands, which have been carefully mapped by M. Cornu, vary in their appearance from day to day and from hour to hour, according to the state of moisture of the air and the height of the sun above the horizon. The co-efficient of absorption for any given ray may be found by measuring its intensity in the solar spectrum at different times of the day, so that the light travels a different length of path through the earth's atmosphere. Photographs of the solar spectrum taken at different times of the day show the growth of the telluric lines. Or, with Professor Langley, we may climb a mountain and look down on the red dust below and take observations of the spectrum and of the deep blue colour of the sky, getting deeper and deeper almost to blackness as we ascend, whilst someone else is observing down below with an extra thickness of half the earth's atmosphere between the two observers. The breadth of the telluric rays and the energy consequently lost by their absorption depend upon the height of the sun above the horizon, and M. Cornu proposes to measure the absorbing power of the atmosphere and the amount of water vapour in it by comparing the telluric lines with the metallic lines near them for equality of blackening. We must distinguish between this purely selective absorption of separate rays, whereby dark lines are produced in convenient positions in the spectrum, and a general absorption by the atmosphere, which has been shown to depend greatly on the presence of particles of matter or dust in it. Larger particles reflect and scatter a beam of light, and treat light of all kinds or colours nearly in the same way. This happens when the size of particles is large compared with the wave length of any kind of light. By these a beam of white light is said to be partially absorbed, but sifting out of rays has been the same for the different kinds of light, and the remainder which passes through is still white light. The smaller particles of dust or mist begin to absorb selectively, being small enough to be affected differently by, and therefore to affect differently, the long and short waves of ether. In this case the short waves of the blue end of the spectrum are absorbed more readily than the long waves of the red end, and in the beam which passes through the blue is wanting. We saw this a few minutes ago in the experiment with resin. As the particles are smaller and smaller, we have less and less absorption in the blue end of the spectrum until a molecule or atom asserts itself by picking out those vibrations of the ether which harmonise with its own special rates of vibration, and leads the transmitted beam with dark lines in the position of its special black absorption lines. These may by the analogy of sound be said to be the special tones of the gas or vapour, and may be distributed throughout the spectrum. Thus for every difference of the small portion of the spectrum there are different values of the co-efficient of absorption by the earth's atmosphere, in consequence of this selective absorption of the various parts. And, as Professor Langley has shown, it will be necessary to measure these special co-efficients of absorption in order to find the true nature of the solar light and heat. The absorption by the earth's atmosphere may be studied by means of artificial lights on the surface of the earth. In mapping the spectra, M. Jansen lit a bonfire and observed the atmospheric lines from a point 13 miles away, and MM. Egoroff and Thollon have determined the order of disappearance, with some of the most prominent absorption lines for different thicknesses of air between Mont Valerien and the observatory at Paris. But it is only in considerable depths that the full effects of the atmosphere in light can be made known. The varieties of colour in the sky at sunrise and at sunset are different to the transmission of the sun's light through varying thicknesses of air full of fine particles of matter and of aqueous vapour, which cut off more or less of the blue rays. Higher and higher above the horizon the thickness of this atmosphere diminishes; more and more blue light of the illuminated haze scattered by reflection mixes with the transmitted light, which gradually diminishes and makes the colour gradually paler until near the zenith there is no longer any transmitted light and the colour of the sky is blue. Just as a sodium flame in front of a weaker flame adds to the intensity of the sodium light, so the blue illuminated haze in front of the blackness of space, or even in front of a dark hill, will give the blue colour to the sky or tinge the hill with blue or violet light.

During the past year interesting experiments have been carried on at the South Foreland Lighthouse on the relative intensities of the most powerful artificial lights with which we are acquainted, and on the general absorption of their light by the atmosphere in different states of clearness. We have only to go back a few years in lighthouse illumination to find as our brightest oil lights either the Fresnel four-wick lamp or the Trinity House four-wick lamp of about 300 candles, giving a cylinder of orange yellow light of about four inches in diameter. By the introduction of the electric light at the South Foreland 15 years ago, under the advice of Faraday, a source of light of 1,200 candles was obtained within a circle half an inch in diameter. This concentration of the light within so small a space is of vast importance in its use behind the lens, and greatly increases the brightness of the light and the distance to which it can be seen. Its advantages to the mariner are shown from the reports of Faraday and Holmes on the comparison of these lights, and from the French report in 1870 of the electric light, which says: "The advantages of the system have been highly appreciated by mariners, and the increase of range of light has been very marked in slightly foggy weather. It certainly enables mariners to continue their voyage and to enter a port at night, when they would not have been able to do so with oil lights."

A comparison of electric and oil lights of the first order was made by M. Petit, hydrographer to the Belgium Government, some years ago, soon after the electric light was permanently established at the South Foreland. He estimates the relative efficiency of the electric light of 1,200 candles and the oil light of 300 candles, and finds that on 76 nights in a hundred the electric light was seen at a distance of more than 20 miles, whereas the strongest oil lights then used were only seen on 29 nights in the hundred at the same distance. Improvements have been made in oil lights by adding extra wings of wicks, bringing the candle-power of the six-wick oil light up to 730 candles and increasing the cylinder of flame to about 4½ inches in diameter. In the meantime gas has also been introduced for lighthouse use, and by the addition of ring after ring of burners, the largest gas lights regularly used give a light of 2,400 candles, giving a cylinder of flame 11 inches in diameter. The oil light 4½ inches in diameter giving a light of 730 candles, and a gas light 11 inches in diameter giving 2,400 candles, will give beams of light which are more and more divergent as the source of light is large, and as seen from any point in the axis of the beam, appear of equal brilliancy when placed behind a very powerful lens. They are nearly equal in brilliancy to the electric light of 1,200 candles in the permanent lighthouse which for the last 14 years has been supplied from the Holmes machine; and in the observations made at sea the three lights have been seen over the same range. Following M. Petit, we may say that both oil and gas have so improved that their practical efficiency has been raised to 76 per cent. But the efficiency of the electric light does not stop at 76 per cent. If we compare the practical efficiency of the permanent electric light supplied by the Holmes machine and the electric light from the De Meritens machines by means of the electric currents which they severally produce, we find in the first case a current of 30 ampères giving an illumination of from 1,200 to 1,500 candles,

and in the second a current of from 120 to 150 ampères, giving a light of ten times the power or from 12,000 to 16,000 candles. The effect of this increase of light would be to render this light visible at sea at a distance of 20 miles, through a hazy atmosphere, on from 88 to 90 out of every 100 nights in the year.

Thus the increase in the illuminating power which is possible with the electric light diminishes the risks to shipping, and raises the practical efficiency of lighthouse lighting from 76 per cent. to 90 per cent., leaving only ten nights in the hundred when the light is not seen 20 miles away. Of the remaining ten nights in the hundred there are some when even the highest lights cannot be seen for a distance of two miles, or even one mile, as has been shown on a few occasions at the South Foreland during the past year. On these occasions the coefficiency of transparency of the atmosphere is so small that an increase of one-hundred-fold in the source of light will hardly increase the distance to which the light is seen by 100 yards. Nor can we be astonished at this when a short time since we read that in a balloon enveloped in a cloud 400 feet in thickness, with bright sunshine all round, the observers were carried long distances in absolute darkness, showing that 100 feet of cloud is quite sufficient to absorb bright sunlight and cause total darkness.

Now let me compare observations made on a very foggy night at the South Foreland on the 12th of February last. A compact six-wick oil light of 730 candles, and a large gas burner giving 2,400 candles, are only just perceptible at a distance of 1,060 feet, and both disappear together. An electric light of 12,000 candles with a smaller lens is seen at a distance of 1,350 feet. The lens for the electric light is smaller, and had the same lens been used as is used for oil and gas, other measurements have shown that the light given out from the electric arc would have been twelve times as great, or that an electric light of 1,000 candles with a large lens would have been seen at 1,350 feet, and that an electric light of 100 candles would have been seen as far as the strongest oil or gas light. That in this statement I am well within the mark will be seen by the fact that at the South Foreland a 50-candle incandescent lamp has given, at a distance of 6,000 feet, an illumination equal to two-thirds of the illumination of the oil or gas. We have here facts which illustrate two very important principles, each bearing upon the efficiency of the electric light as regards its power of penetration of the earth's atmosphere when used behind the lens. The oil light of 730 candles gives a light which, because of its compactness, is equal in illumination at any single point to the gas light of three times the intensity; and the still smaller and more compact electric light, giving a beam more nearly parallel, gives the same illumination. The question may well be asked, How is it that any electric light of 100-candles will penetrate a fog to a distance of 1,000 feet while ten times the light of the same kind will only penetrate 350 feet farther?

Let me try to make clear to you the law of absorption of light by the atmosphere, or by any other absorbing medium. Take a beam of light in its course through successive portions of atmospere of equal length. Each successive portion will not absorb the same amount of light, but each successive portion will absorb a given fraction of the light which falls upon that portion, and the quantity absorbed by each will depend upon its distance from the source. If, for instance, the first portion absorbed one-half the light, the second portion will not absorb all the remainder, but only one-half of it—that is, a quarter of the whole. The next portion will not absorb the remaining quarter, but only one-half of it—that is, one-eighth of the whole—and so one-eighth of the whole will have passed through these three portions. Now let us apply this law to the case of our electric light of 100 and 1,000 candles. Take the case represented by this table, the figures in which I have taken from determinations at the South Foreland.

Illuminations at different Distances from Source A *and* B *of* 12,000 *and* 2,500 *Candles in a Fog (Coefficiency of Absorption* ·9537 *per foot.*

Distance in Feet.	Intensity of A.	Intensity of B.	Intensity of 1,000 Candles.	Intensity per 1,000 Candles with no Absorption.
	12000	2500	1000	1000
10	74·68	15·560	6·223	10
20	11·64	2·425	·970	2·500
30	3·21	·670	·268	1·111
40	1·12	·234	·094	·625
50	·45	·093	·037	·400
60	·19	·040	·016	·278
70	·09	·018	...	·204
80	·04	·009	...	·156
90	·02	...	...	·123
100	·01	...	...	·1

It will be seen that in the first column we have distances in feet measured, in the second intensities of light and with a certain coefficiency of absorption, ·9537 per foot; that is to say, that fraction of the light from the source extending through a distance of one foot from it. We have the numbers in those columns representing the quantity of light which will reach to the respective distances. For instance, 12,000 candle light with that coefficiency of absorption would give a light of between 74 and 75 candles at a distance of 10 feet. Its intensity would be reduced to the $\frac{1}{100}$th part of a candle at a distance of 100 feet, and in that state of fog which was then contemplated that would be the smallest light which could be seen, ·01, representing the limiting value of the light which could be seen through such a fog; so that here we have represented the distance to which that light is seen as 100. If we look at the other headings of the columns we have 2,500 for the intensity of the whole original source, and we see that 80 feet is the distance to which that light would be seen; 77 is about the number corresponding to that table. If we take a thousand candles as our source, the numbers in the third column represent the intensities at different distances, and the light would be seen to a distance of 65 or 66 feet. We may see from that second column how far the light of one single candle would be seen. For instance, opposite 40 we have 1·12, a little more than one candle; that is reduced to ·01, or to the smallest light which we can see at the distance of 100. The light of one candle, then, would be seen to distances of something less than the difference between those two numbers; that is to say, a little less than 60 feet. The fact is, the number would be 55. A single candle with that state of fog would be seen at a distance of 55 feet, whereas 12,000 candles are only seen at a distance of 100 feet. That case will, I think, illustrate the law of absorption, and will show that we have there a law which contemplates something else than diminution inversely as the square of the distance. To these cases of absorption that law does not apply.

I must pass over some points which are especially interesting to those who take a great practical interest in this subject as to the distances to which lights are seen when they are of great service, that is to say, in cases where the fogs are not so thick as those of which I have been speaking.

In order to study the effects of very dense fogs in eclipsing bare lights, steam was allowed to issue from the steam pipes introduced into the photometric gallery, 380 feet in length, at the South Foreland lighthouse. The steam was let out at various points so as to fill the gallery with a dense and fairly homogeneous mass of condensed steam, the supply being kept up from points further away from the lights than the observer was. Such a mixture of air and condensed steam might fairly be taken to represent a dense sea fog or a genuine Scotch mist. Let me give you the results of some of my experiments made in this gallery. At one end of the gallery we placed a gas burner, giving an illumination of 2,500 candles, and a De Meritens arc lamp, giving a light of about 12,000 candles. These were alternately eclipsed by placing a screen in front of first one and then the other. Such was the density of the fog which could by this means be obtained that in the gallery, when daylight was shut out, at a distance of more than 100 feet, I could see no light at all, nor even the slightest illumination of the fog, and the eclipsing or exposing either light made not the slightest change whatever in the state of the gloom. No light sufficient to affect the eye penetrated the fog to a distance of more than 100 feet. From 90 feet to 100 feet, when the electric light was exposed there was just the faintest possible illumination of the fog in the general direction of the source of light, but when the gas was exposed not the slightest illumination could be seen. On approaching nearer the illumination from the electric light became brighter; but when that was eclipsed there was no illumination whatever from the gas until a point 65 feet from the source of light was reached. From 60 to 65 feet there was just a faint illumination of the fog from the gas light. At this distance the fog was well illuminated when the electric light was exposed, but the source of light, the electric arc itself, could not be seen. On approaching nearer, at 50 feet, the glowing 40-millimetre carbons became visible, not as a single point of light, but of the full breadth of the carbons. On approaching still nearer the gas flames, these were not seen until at a distance of 36 feet the whole breadth of flame came into view at once, and the direction of the source became accurately defined.

In these experiments with the fog in the photometric gallery, my observations agree with those made by Mr. Dixon, and taking the value which he gives for the relative distances at which the electric light and gas were seen I have formed that table of absorption to which I have just alluded.

An interesting experiment was made with the electric light of 12,000 candles and the standard single candle in the steam fog. It was found that the glowing carbons were seen to a distance of 80 feet, and the single candle standard was seen to a distance of 40 feet. Again, in another experiment the glowing carbons were seen to a distance of 117 feet and the standard candle to a distance of 55 feet, whereas the gas light of 2,500 candles was seen to a distance of 80 feet. A single gas jet in this steam-fog was seen to a distance of 70 feet, that is, within 10 feet of the light of 2,500 candles, showing that the distances to which the light is seen in fog depends on the intrinsic brightness of the flame, and that an increase in the size adds very little to the penetrative power.

The observations made as to the character of the illumination in these dense fogs are very important in connection with the subject of my lecture. When first the faintest illumination from the electric light became visible at from 90 to 100 feet, the illumination was not red or yellow, but white, like moonlight, and at any and all distances there could be no question whatever as to the source which was giving the light. Whenever the electric light was exposed the illumination of the fog was white, but whenever the gas was exposed the illumination was yellow or orange yellow; the illumination of the fog was of the same colour as the source of light, showing that with a mixture of air and vapour in the form of steam, forming a cloud, without a mixture of other substances,

rays of different colours were absorbed and scattered in very nearly the same proportion. During these experiments with steam a pocket spectroscope clearly revealed the rain-bands in the yellow near the sodium lines and other rain-bands in other parts of the spectrum, showing that the true selective absorption of the molecules of water vapour was not specially confined to the blue end of the spectrum.

What, then, may be gathered from these experiments at the South Foreland as to the relative effect of the electric light in lighthouses as compared with other lighthouse lights? The experiments have shown that, using the same kind of lens for all kinds of lights—using large lenses for gaslights of greatest power and the smaller lens for the electric light—the electric light in clear weather was at least sixteen times as bright, and in foggy weather was thirteen times as bright as the gas. The experiments have also shown that that the electric light is at least of twelve times the intensity with the large lens as with the smaller lens. Hence the illumination which will be given to the mariner by the electric light, using the same lens as the gas, is from 150 to 250 times as great as that given by gas or oil.

If we take lights of the two kinds of equal intensity, the electric light in thick weather will give the mariner 30 times the light and in clear weather 40 times the light given from the other sources. With regard to the question of the absorbing power of the atmosphere in different kinds of weather for different lighthouse lights, the principles I have attempted to explain as to the greater absorption of the atmosphere of the blue rays than of the red, and the fact that the electric light while abounding in all is especially rich in red, would lead us to expect that the illumination from the electric light will be weakened somewhat more than that from other sources in foggy weather. Although this does not appear from the steam fogs, yet the other measurements made at the South Foreland show a somewhat greater weakening of the electric beam.

Prof. STOKES, of Cambridge, rose at the call of the President to ask the meeting to return a vote of thanks to Prof. Adams for his elaborate lecture. The subject to which Prof. Adams invited their attention in the latter part of the lecture illustrated the remarks of their President in his opening as to the industrial appliances of science. It was a matter of the utmost moment with respect to the preservation of life and property that the mariner should be warned at sea of his approach to the land, and informed by the lights exhibited to him of the land he was approaching. There was one great difficulty in the way of carrying out that idea, and that was the prevalence in certain cases of fogs, which rendered it almost impossible to see a light until the mariner was perhaps dangerously near to the part of the coast on which it was erected. The experiments accordingly which were performed by the Lighthouse Commissioners and the Trinity Board were of great importance for practical purposes. By the favour of the Commissioners he (Prof. Stokes) was enabled to inspect the apparatus, and he witnessed those experiments to which the lecturer referred of producing an artificial fog by means of steam let into a long gallery. The result of these experiments seemed rather contrary to what people had been led to suppose beforehand—namely, that the electric light would lose more in proportion than the other sources of light by the fog. Such was not found to be the case, at least with the artificial fog which was produced under the circumstances referred to. There was reason, he thought, from theoretical grounds, to suppose that the different kinds of light, notwithstanding their difference of colour—the electric light being much bluer than gas or oil—would be weakened in the same proportion by a fog composed of globules of water—that is, if the globules were of a tolerable size. If, however, the globules were excessively small, it was possible that the case might be different. These excessively small globules might occur also in the case of haze, where, for example, the light of the sun when near the horizon was seen through a long stretch of air. That was not the sort of fog that was formidable to the mariner; and he thought it probable that the really formidable fogs agreed, or at least agreed to a very considerable degree, with those artificial fogs which were produced in the experimental gallery by letting in a jet of steam. The appliances which he was permitted to witness were of the most elaborate description for the measurement of light and everything relating to the inquiry, showing how science might, as he remarked before, be brought to bear on the practical concerns of life.

Sir WILLIAM THOMSON said it had been indeed a great comfort to himself personally to have heard what they had heard that night. He well remembered the feeling of discouragement of those who were interested in electricity and wished to see the success of electric light, when they were warned that the electric light was only of use for dazzling the eyes of people when very near to it, and was not available for penetrating to a great distance. He believed that ill-omened judgment with regard to the electric light was formed from the seeing of the electric light on the Thames Embankment through a London fog. He scarcely thought this remark would apply to Aberdeen, but the atmosphere of London and Glasgow, and perhaps Manchester, had a very peculiar character, not known here, and long might it remain unknown. It made all lights red at a very short distance. Gas lamps looked red at the distance of half a mile or a quarter of a mile. In such circumstances the electric light was shorn of those rays in which it abounded chiefly, and was reduced almost to the dull power of gas lamps. He had himself seen the electric lights on the Thames Embankment scarcely discernible from gas lights at no greater distance. And that was not the case of fogs that were dangerous to mariners; it was not on the Thames Embankment that lighthouses were needed; but the fogs that sailors suffered from were essentially white fogs, and these, as had been explained, did not cause a much greater, or perhaps not greater at all, absorption of electric light. The enormous power of the electric light, which dazzled one when seen near, was not for the use of the mariner, and when they could see it for even 40 miles distance in moderately clear weather, it had also proportionally great power when seen through a fog. He believed that at the distance of 20 miles the shadow of telegraph poles could be seen by the light, and at the entrance to Havre Harbour it shone with immense brilliance 28 miles away. Even in a clear night it was a very great comfort to the mariner to know 10 miles further away what his precise position was. He had great pleasure in seconding the motion to give a cordial vote of thanks to Prof. Adams for his lecture.

Prof. ADAMS briefly expressed his thanks, and the proceedings terminated.

INAUGURATION OF THE ELECTRIC TRAMWAY AT BLACKPOOL.

THIS electric tramway, of which Mr. M. Holroyd Smith is the inventor and engineer, was formally dedicated to public use on Tuesday, amidst the most enthusiastic rejoicings. The Lord Mayor of York, the mayors of Manchester and Liverpool, and of about twenty-five other towns, accepted the invitation of the Mayor of Blackpool to do honour to the occasion, and vast crowds poured in by ordinary and excursion trains from all the large towns in Lancashire and Cheshire. It is estimated that about 50,000 persons were present. The proceedings of the day commenced with a procession of bands, volunteers, fire brigade, officials and council of the town, life-boats, carriages containing the mayors and the invited guests, and a number of trade groups, &c., intended to add to its interest and effect as a popular demonstration.

The ceremony of inaugurating the tramway was initiated by Alderman R. Horsfall, of Halifax, Chairman of the Blackpool Tramway Company, who expressed his pleasure at that day seeing the first electric tramway in England opened for public traffic. He admitted that the company had had a great many difficulties to encounter, but he believed they were now virtually surmounted, and that they would be able to show that an electric tramway, which many people had said was an impossibility, was not only practicable, but that it would be a benefit to the town of Blackpool.

Mr. Holroyd Smith, being called upon to make a short statement, said it afforded him both pleasure and pride to feel that that day was to witness the inauguration of the electric tramway system in England. He felt it would be the most memorable event of his life, believing as he did that the new enterprise, of which he was the inventor, would do much to benefit mankind. Electricity he compared to a wilful child, requiring great care and watchfulness in its application to practical purposes; but he hoped they had so far succeeded with regard to this particular work as to apply it in such a way as to be not only efficient, but reliable, in the interest of public safety. Mr. Holroyd Smith then handed to the Mayor of Manchester a starting lever in ebony and brass, on which was inscribed, "Presented to Alderman Harwood, Mayor of Manchester, by Mr. Holroyd Smith, engineer, on the occasion of his Worship inaugurating the Blackpool Electric Tramway.—September 29th, 1885."

The Mayor of Manchester, before making the necessary connection to start the car, complimented the engineer on the success attaching to this stage of the enterprise. He also complimented the shareholders, because, although their investment might not be immediately remunerative, they would at least succeed in that which they had more in view than the question of profit, namely, the adding another inducement to the hard-worked people of our large towns and centres of industry to come to Blackpool to repair their wasted energies, and find health and enjoyment. He had great pleasure in declaring that electric tramway open, and dedicated to the use of the public. His Worship then applied the lever, and set the car in motion.

A vote of thanks closed the formal proceedings, after which the car was driven by the mayor along the Promenade.

In the afternoon, at the instance of the Mayor and Mayoress of Blackpool and the *fête* committee, a collation was served to the invited guests at the Borough Hall, at which the mayor (Alderman Cocker) presided. Amongst other toasts was, "The latest achievement of modern science—the electric tramway—may it prosper," which was proposed by Mr. Daniel Adamson, who is just now the most prominent man in Manchester, owing to the success of the Ship Canal scheme. In a vigorous speech Mr. Adamson referred to the opening of the Stockton and Darlington Railway, in 1825, comparing that occasion with the present; and expressed the hope and belief that the inauguration of the Blackpool Electric Tramway would prove an equally auspicious and portentous occasion.

Alderman R. Horsfall, chairman of the tramway company, in reply, expressed the thanks of the company for the attendance of so many gentlemen of position and influence at the opening festivities, and reviewed the history of the rapid rise of Blackpool, which now, on account of both its natural and its added attractions, stands in the front rank as a health resort. He looked upon the

electric tramway as another addition to its attractions, and one that would bring visitors next season from all parts of the country.

Mr. Holroyd Smith, the electrical engineer of the company, also replied to the toast, expressing the great pleasure he felt in the fact that it had been proposed by Mr. Daniel Adamson, and thanking the assembly for the cordial manner in which they had received it.

A subsequent speaker claimed for Blackpool that it was the first watering place in the country to adopt the electric light.

The day's demonstration concluded with a display of fireworks on the sea, lasting till nearly midnight.

The tramway line of Blackpool is about two miles in length, and consists of the ordinary tram rails with the addition of a central channel sunk below the roadway, and presenting only a very narrow slit or opening to the surface. Within this channel are laid the two electric conductors, made of special drawn copper, and so placed against the sides of the channel as to be protected from the possibility of injury being occasioned to them through the slit at the top. This arrangement is also necessary to enable points, loops, and crossings to be treated effectually. Since we described and illustrated the experimental tramcar and track, which Mr. Holroyd Smith constructed at Manchester in 1884 (ELECTRICAL REVIEW No. 355), he has introduced improvements into almost every detail, tending very much towards the achievement of that end which he has always kept in view—of being able to employ the electric current for the propulsion of street cars without endangering the safety of the public, and with a fair prospect of a remunerative dividend to those who should take up the matter as an investment for capital. In this connection it may be mentioned that the shareholders of the Blackpool Electric Tramway Company are very sanguine as to the undertaking being a financial success, even in the first year of its working, though it would appear that this has not been the chief object of the promotors, who have taken up the matter as a labour of love, the directors giving their services gratuitously.

The plant consists of two very fine 25 H.P. engines by eminent makers, which each drive a large dynamo specially made by Messrs. Elwell-Parker for this work. It is calculated that the current generated by these means is sufficient to drive ten cars fully loaded with their 400 passengers. The cars are of the ordinary appearance, the motor being entirely hidden in the centre of the flooring. Considerable ingenuity has been displayed in so simplifying the arrangements for controlling the speed and direction of the car that an ordinary unskilled driver has very little to learn. The starting gear consists merely of two handles or plugs, which fit into four holes arranged like the points of a diamond—if the car is to move forward he puts the handles in the top and bottom holes, if the car is required to move backwards he removes them into the two side holes. The speed is controlled in a similarly common-place manner by a lever, and a little practice in the use of this, and of the break, is all that is needed to render the driver perfect in his duties. On a future occasion we hope to be able to describe and illustrate an important improvement in the matter of switches and other technical details which Mr. Smith has recently invented, and also to give an illustration of the engine and dynamo room of the Blackpool Tramway Company.

OBITUARY.

ON Tuesday, September 22nd, the remains of Mr. John Muirhead were interred in the Cemetery of Lower Norwood.

The deceased was in many respects a remarkable man; but such was the quietude and modesty of his character that few, except the members of his own family, had any real knowledge of the good and sterling qualities he possessed.

Mr. Muirhead was one of the last survivors of the early days of the electric telegraph. He was the colleague and fellow-worker of nearly all the men whose names are indissolubly associated with the introduction of that great invention, and very interesting were his reminiscences of the old times. But he was a man not much given to talking, and most of his recollections have ended with him. Born at Gifford, in Scotland, in the year 1807, his life has extended over what will probably be considered as one of the most progressive ages in the world's history. The more material part of this progression has been the development of the railway and the telegraph; and in the latter of these great works the late Mr. Muirhead has played an honourable part. The electric telegraph, under Cooke and Wheatstone, had begun to take a practical form in this country in the year 1837, but it was struggling with many difficulties when in 1846 the original Electric Telegraph Company was founded. Mr. Muirhead, who even as a young man showed a decided talent for construction, afterwards joined the company, and remained in its service until the system was transferred to the Post Office in 1870. From his first connection with the telegraph until within a few years of his death Mr. Muirhead devoted himself continuously to the work of extending the system, building the lines and perfecting the appliances. At this time the cost of a message, including addresses, between London and Edinburgh was twelve shillings and sixpence; and had Mr. Muirhead lived a few weeks longer he would have seen the same service performed for sixpence. The tubular bracket of annealed iron which he introduced in place of the solid cast iron one hitherto employed was rapidly adopted and is now in general use. The hollow iron pole which under various improvements is now so common was also, we believe, one of his ideas. The early Metropolitan lines were laid by him, and the pneumatic system for collecting messages from the local telegraph stations to the central transmitting station in London and other provincial towns was first installed under his supervision. Mr. Latimer Clark and the late Mr. Cromwell Fleetwood Varley did much to introduce and perfect this novel system of communication, which is now employed in all the important telegraphic centres. The plan of using compressed air and sending the telegrams both ways in the tubes was probably due to Varley; but the many details which form important elements in the successful working of the system were due to Muirhead. In the absence of manufacturing facilities such as exist now for the supply of telegraphs to the world at large, Mr. Muirhead in conjunction with Mr. Latimer Clark and Mr. W. M. Warden, of Birmingham, founded the house now known as Latimer Clark, Muirhead and Company, a firm which in the course of its career has planted the telegraph in every corner of the world and doubled the carrying power of nearly all the great submarine cables by the duplex system, which is the invention of Mr. Muirhead's sons. It was from this manufactory that Mr. Muirhead introduced the form of battery which bears his name. Other improvements and innovations of Mr. Muirhead might be mentioned, but the greater part of his time was devoted to the practical execution of telegraphic works. His ordinary duties took him to every part of the country, and left him very little leisure time. Nevertheless, he was a well-read man, and on retiring from business a few years ago his favourite amusement was reading works of history, a branch of study in which he was extremely well versed. His knowledge of events and their dates was accurate and comprehensive. Endowed with a very strong and robust frame he hardly knew a day's illness, except at rare intervals, when overwork or exposure had told upon his constitution. His natural vigour was sustained by sober habits, both of mind and living. A few months before his death the health which he had hitherto enjoyed began to break down, and the tranquil leisure he had earned so well was interrupted by a serious illness. He rallied to some extent from this, but on Thursday, the 17th September, while conversing quietly with his doctor in the library of his residence at Oakwood, he suddenly fell back and expired.

Mr. Muirhead was unquestionably a successful man; and his success was due rather to the influence of his character than to exercise of genius. His workmen liked and respected him because of his practical knowledge of the business and his kindly interest in them and their affairs. Under a somewhat rugged exterior he concealed a warm and kindly heart. But such was his dislike of anything approaching to ostentation, that most of his benevolence was strictly private, and perhaps only known to the recipient. He had a sympathy with struggling men, especially artizans, and enjoyed giving them a helping hand in his own works. Some of these turned out badly, others well, and a few have even become notable. He believed firmly in education for the masses. "A good education," he was wont to say, "is much better than money; a man cannot pawn it."

Living an upright and faithful life, he has left few or no enemies behind him. He was one of those who never sought to shine at all, and most of his work was of that silent order which is its own praise.

REVIEWS.

Manual of Telegraphy. By W. WILLIAMS, Superintendent Indian Government Telegraphs. London: Longmans, Green & Co.

This manual, the author states in his preface, "has been written by order of the Director-General of Telegraphs in India, and is based on the substance of the papers on technical subjects, set by the author at the general examinations of the Indian Telegraph Department." Although there is much that is commendable in the book the author seems to be behind the times; it is certainly surprising that in a manual issued towards the end of the year 1885 we should find such a statement as that made on page 5, viz., that "the unit of quantity (!) or current is called an Ampère, Weber, or Oerstedt." The subjects embrace a general description of the various instruments, batteries, and circuits with which an Indian telegraph official may be called upon to deal with. With reference to batteries, we would point out that the common mistake of calling the Minotto element the Minotti is committed; the correct spelling (a small matter certainly) of this name was definitely settled a few years ago by a letter which appeared in our columns, written by the Chevalier D'Amico, Inspector-General of the Italian Telegraph Administration. The methods of dealing with faults is also gone into; the conservancy of, and mode of testing lines, batteries, instruments, lightning conductors, and earths are also dealt with. In many respects, the work of Mr. Williams may be considered as an abbreviated edition of the more elaborate treatises of the late Mr. Schwendler; and it will no doubt supplant the latter work, and with advantage. The volume is, of course, of more value to those engaged in telegraph work in India than to employés in this country, though, as we said before, there is much in it that is commendable.

A Pocket-Book of Electrical Rules and Tables. By J. MUNRO and A. JAMIESON. Third Edition. London: Charles Griffin & Co., 12, Exeter Street, Strand.

Several important additions have been made in the new edition of this popular pocket-book, including an appendix by Prof. Ewing on Magnetic Measurements. It is unnecessary to say more with reference to the work than to congratulate the authors on the success which they have achieved.

NOTES.

Lighting of the "Polyphemus."—Messrs. Goolden and Trotter inform us that our Portsmouth correspondent was incorrect in his statement last week that H.M.S. *Polyphemus* was lighted by them; the ship was lighted throughout by Messrs. Siemens Bros.

Electric Lighting in New Zealand.—At the New Zealand Industrial Exhibition, held at Wellington in August, Messrs. R. E. Fletcher & Co., of Dunedin, had a varied collection of apparatus for electric lighting. Three arc lights were suspended from a 100 feet mast in the grounds, and several suites of rooms were lighted by Messrs. Fletcher & Co. with Swan incandescent lamps. The same firm has fitted up the Mosgiel woollen factory with a Victoria dynamo machine for 300 lamps, this being the largest incandescent installation in the colony.

Sixpenny Telegrams.—On enquiry being made on the 1st inst. at some of the leading telegraph offices in London, it was stated that up to noon the increase in the number of telegrams sent under the new sixpenny rate amounted to about 50 per cent.

Economical Lamps.—It is stated that the Oppermann 16-candle-power incandescent lamp for the lighting of Tours requires but 0·9 ampère at an E.M.F. of 40 volts. This represents just 2¼ watts per candle-power, but probably the total luminosity of each lamp is nearer that of 12 candles.

Electric Lighting in Philadelphia.—At the meeting of the American Association, which has just terminated, a paper was read by Prof. Elliott upon electric lighting, giving the results of an examination of the system in use in the Philadelphia post office. The system is the Weston; the incandescent lights employing an electromotive force of 73·75 volts, and the arc lights a current of 80·05 ampères. One effective horse-power of the engine was required for 13·25 incandescent lights, and for 1·43 arc lights. One horse-power on the incandescent circuit gave a light equal to 237 standard candles, and on the arc light circuit of 1,077·3 candles. In remarking upon these results, a member called attention to the low electromotive force employed, and pointed out that this largely increased the safety of the system as compared with others.

Train Lighting.—The *Financial News* says: "The lighting of the carriages on the District line is so abominably bad that it has become a positive scandal. If it is not improved passengers by the line will be compelled to carry lanterns with them, or perhaps some enterprising genius will furnish them with a portable District Railway electric light at a low price."

The Bell Telephone Patent.—The most sensational evidence in the case pending between the National Improved Telephone Company and the American Bell Telephone Company, in which the former have the assistance of the Government, will be as to advantages said to have been gained by Bell through the action of the examiner at the Patent Office and his superior officers. According to the *New York Times* the bill asserts that Mr. Bell was enabled to secure his first patent by means of information unlawfully communicated to him or to his attorneys by an officer of the Patent office. This information related to the contents of a caveat filed by Elisha Gray, of Chicago. Bell's application for a patent was filed on February 14th, 1876, and on the same day Gray's caveat was filed. The application was assigned to Z. F. Wilber, an examiner in the office. The law requires that a caveat shall be placed in the "confidential archives," and shall be "preserved in secrecy." But Wilber informed Bell or his attorneys that Gray's caveat had been filed, and that three claims in Bell's application related to matters specified in the caveat. He also informed him that on this account the application had been suspended for ninety days. Thereupon Bell, it is alleged, convinced Wilber that although the application and caveat had been filed on the same day the application antedated the caveat an hour or two. For this reason the suspension was revoked. Then Bell, it is said, amended his application in a manner suggested by the information concerning Gray's caveat, which he had obtained from Wilber. The amendments were allowed, and a few weeks afterward the first Bell patent was issued. The court is asked to repeal, recall, or cancel the patent, because it was procured wrongfully and contrary to law. Among the exhibits submitted by Mr. Van Benthuysen and his associates is the affidavit of Wilber, the examiner.

Promotion of Telephone Officials.—We note that Mr. James Townsend, who has for the last twelve months been at Inverness as the district manager for the National Telephone Company, has been appointed to the newly-formed Scottish South Central District, which embraces the counties of Fife, Kinross, Clackmannan, and Stirling, with headquarters at Kirkcaldy, a promotion which we do not doubt will result beneficially, both to Mr. Townsend and the company who employs him. Mr. R. C. Bennett, who has been assistant at Inverness since the opening of the exchanges, is the new manager for the vacated district.

Telephone Statistics.—The following table was given in a paper read by Mr. B. E. Sunny before the recent Telephone Convention in New York. It shows the number of subscribers to the various exchanges, and the ratio of persons to every one telephone :—

	Population. Census 1880.	Ratio.	Subs., 1885.	Increase over 1884.
Baltimore	332,313	204	1,630	180
Boston	362,535	214	1,696	...
Brooklyn	566,663	241	2,354	508
Buffalo	155,134	101	1,533	113
Chicago	503,185	138	3,630	404
Cincinnati	255,130	101	2,535	297
Cleveland	160,146	98	1,620	3
New York } „ Law }	1,206,209	229	5,252	444
Philadelphia ...	847,170	366	2,310	111
Pittsburgh... ...	247,765	148	1,688	...
St. Louis	350,518	229	1,528	156
Albany	90,758	77	1,158	16
Detroit	116,340	59	1,956	256
Louisville	135,000	106	1,271	...
Lowell	59,485	74	800	...
Milwaukee	115,587	84	1,375	212
Paterson	51,031	78	651	100
Providence... ...	104,857	48	2,162	270
Rochester	89,366	110	808	82
Worcester	58,295	75	777	...
Washington ...	144,293	127	1,138	366
Jersey City ...	120,722	187	648	247
Denver	35,629	...	751	28

Telephonic Extension. — The National Telephone Company has secured a sufficient number of subscribers in Galashiels and Selkirk to enable it to introduce its system. The two towns are to be connected. The building of the lines will be proceeded with shortly.

A Western Australia Cable.—The telegraph cable for connecting Breaksea Island with King George's Sound has been successfully laid. The work of signalling vessels passing Breaksea Island, and reporting the fact by means of the cable, will be inaugurated in the course of a short time.

Theft of Telegraph Wire.—It was recently discovered at Conshohocken, Pa., that three unknown thieves had mounted the telegraph poles from West Conshohocken to Bala Station, a distance of seven and a-half miles, and had stripped and stolen two lines of copper telegraph wire of the Postal Telegraph Company. The total length of wire stolen was fifteen miles, weight nearly a ton, and value £100. The thieves operated in broad daylight, passing along the road in a waggon, removing the wire and acting as if they were authorised *employés*.

Electricity in the Navy.—Some important experiments are to be made at Portsmouth by the *Hercules* which, with her new machine guns and ordinary artillery, will practice on a shore battery, with the view of ascertaining the exact destructive effect of the guns of a vessel when she is going at full speed. The shore battery will reply with blank cartridge, which are to be fired by electricity.

Experiments with Engines.—In the October number of the *Franklin Institute Journal* may be found the following interesting papers :—"An Account of the Experiments made on a Condensing Compound Engine, by a Committee of the Industrial Society of Mulhouse, in Alsace, Germany. By Chief Engineer Isherwood, U.S.N." "Cylinder Condensation in Steam Engines: An Experimental Investigation. By Chas. L. Gately, M.E., and Alvin P. Kletzsch, M.E., with an introduction by Prof. R. H. Thurston."

The Microscopic Structure of Iron and Steel.—In May last, Mr. F. Lynwood Garrison read a paper at the Chattanooga meeting of the American Institute of Mining Engineers. This is now published in the October number of the *Journal of the Franklin Institute*, and illustrated with magnificent plates. We would call the attention of all makers of dynamos to this important paper.

Wire Gauges.—The report to the Telephone Convention which recently met in New York on wire gauges stated that out of 15 replies from manufacturers of wire only two favoured the American gauge; six showed no preference, and the other seven were in favour of the English standard gauge. As we stated last week, it was resolved to adopt the English gauge.

Electric Motors.—The equipment of the New York Elevated Railway on the Daft system has, the *Electrical World* asserts, roused the projectors of new elevated roads in the States to equip them at once on the electric system. The promotors of a railway of the kind in Boston have made arrangements with the Daft Company for the introduction of its motors. Our contemporary has seen the report of a disinterested party who has investigated the Daft railway at Baltimore, which report is a flattering testimonial to the success of the road.

The Invention of the Dynamo.—It is not a little strange that he who first provisionally protected the idea of self-exciting dynamos in this country should be the same man who so clearly described compound winding in the specification from which we made extracts last week, but who laid no claim to that special kind of coiling.

Electricians as Legislators.—The extension of voting power to totally new classes has led to the appearance as candidates for Parliamentary honours of men of all sorts and conditions. In the crowd of mediocrities who are seeking notoriety, it is to be hoped that the men of sterling qualities and sound practical knowledge will not be overlooked and preference be given to the more assuming candidates who are politicians wholely and solely. We have already had to chronicle the aspirations in this direction of several gentlemen connected more or less closely with the interests which this journal follows; and now we have to note the candidature in the East of London of an eminent electrician in the person of Sir David Salomons. Sir David, who is now wooing the electors of St. George's-in-the-East in the interests of Liberalism, is yet comparatively young, having been born in 1851, but his achievements have been such as to justify us in devoting some space to an enumeration of a few of the most noteworthy features of his career. He lost his parents early in life, but his guardianship was undertaken by the late Sir David Salomons, M.P., whom he succeeded in the baronetcy in 1873. His education was completed at Caius College, Cambridge, where he graduated in the Natural Science Tripos, and took his M.A. degree, his inclination being more to physical science than to pure mathematics. At his country seat at Broomhill, near Tunbridge Wells, he has a complete laboratory, where he devotes himself to study and experiment, especially in the development and application of electricity. He is a Justice of the Peace for Kent, Sussex, Middlesex, and Westminster, and a Deputy Lieutenant and Income Tax Commissioner in Kent. He has been active, as our readers know, in scientific labour for a number of years, being an Associate of the Institute of Civil Engineers, a Fellow of the Royal Astronomical Society, a Fellow of the Chemical Society, and a member of the Society of Telegraph Engineers and Electricians. His name may frequently be found appended to contributions to the columns of this journal. He contested Mid-Kent in 1874, but was defeated. Since that time he has occupied himself with art and science, and at his country residence at Tunbridge Wells has devoted much time to the development of electrical science, and the perfection of detail, in division, sub-division, and prompt control, with the result that he has now one of the most complete electric lighting installations in existence. His house, with its sculpture court and picture gallery full of choice specimens of art, is beautifully illuminated with the steadiest method of electric illumination obtainable, and electricity is applied to every possible purpose, even to marking the score at billiards

and working the sewing machine. In his workshops are machines for various purposes, all driven by an electric motor—machines for drilling, milling, shearing, sawing, grinding, and planing; and lathes of various dimensions, with the accompanying requisites for work either rough or delicate. Outside these buildings is a tower about 100 feet high—the observatory—which is fitted with electric light arrangements, &c., including a lamp of 10,000 candle-power. Prior to turning his attention to electricity, Sir David Salomons was attracted to photography, among other things. He has a laboratory especially for photographic processess, and as the result of his work in this direction, has gained acknowledgment as an inventor on several occasions. For instance, as far back as 1873, he exhibited at the International Exhibition his nitro-ammonia process, which preceded the introduction of the dry plate, and an electric exposing camera invented by him in 1870. There can be no doubt that legislation in past times has frequently suffered through the absence from Parliament of men of practical minds, and it is highly desirable that, as we are to witness a revolution in Parliamentary representation, the worth of men with a record like that of Sir David Salomons, independently of the particular shade of party politics they may affect, should be fully recognised and appreciated by the newly-enfranchised as well as the old electors.

Charterhouse Science and Art Schools.—The winter session of this, one of the largest Science and Art Schools in the United Kingdom, will, under the presidency of the Rev. Henry Swann, M.A., commence to-day. During the late session about 800 students, mostly elementary teachers, availed themselves of the privileges afforded by this institution, and of this number upwards of 500 presented themselves for examination, and were successful in obtaining a large number of first-class certificates, and also a goodly number of honours certificates awarded by the Science and Art Department of South Kensington. The only Royal Exhibition taken by a London student this year was by a member of this science school. At a very nominal fee instruction of a practical character is given in most of the sciences; and students who aim at becoming proficient in chemistry (organic and inorganic) have the opportunity of working in a well fitted laboratory, capable of accommodating sixty persons. Aspirants for university honours can, at a small expense, be assisted in their studies. Full particulars can be had of the organising secretary, C. Smith.

The Berthoud-Borel Cables.—The Imperial Austrian Ministry of Commerce state that the last tests made by the Department of Posts and Telegraphs at Insbrück, have proved that the Berthoud-Borel cables for the Arlberg Tunnels remain, both as regards conductivity and insulation, in a condition completely satisfactory.

Submarine Duplex. — Dr. Muirhead has recently returned from America after successfully duplexing the New York to Canso (Nova Scotia) section of the Mackay-Bennett Atlantic Cable system. An official trial of the duplex as pitted against the ordinary method of cable signalling resulted in showing a gain in speed of 100 per cent. by the former method. By a recent improvement introduced into the Muirhead system the retardation due to the differential or bridge part of the arrangement has been reduced almost to nil, and on the above cable the daily variations of the balance are instantly compensated by a new device. The whole of the Commercial Cable Company's system is now worked by Muirhead's duplex apparatus; that is to say, a total length of about 6,700 knots.

Science Lectures. — Under the auspices of the Greenock Philosophical Society, Mr. Wm. Lant Carpenter, B.A., B.Sc., F.C.S., of the School of Electrical Engineering, London, will on the 5th inst. deliver an illustrated lecture on "Electrical Energy and its Uses." On the 19th a second lecture on "The Telephone, or Talking by Electricity."

Electrical Engineering at King's College.—Prof. W. Grylls Adams, F.R.S., will deliver a course of lectures on Heat and Light during the academical year 1885-6. A course of practical work in electrical testing and measurement with especial reference to electrical engineering will also be carried on under his direction in the Wheatstone laboratory. The lectures will be given once a week—on Mondays at 2 p.m.—and the laboratory will be open on Wednesday and Friday from 1 to 4. There will also be courses of lectures on Mechanics and the Principles of Energy. For further particulars apply to Prof. Adams, King's College, London.

Electric Light and Science Teaching.—Several long letters have recently appeared in the *Glasgow Herald* upon this subject, "Electrician" attributing the fiasco in electric lighting matters of two or three years ago to the popular ignorance of electrical science, which left the public at the mercy of company-mongers and foolish enthusiasts, who promised impossibilities which are still being waited for, to the detriment of genuine industry. He says that if our people had in the past been taught something of science, as the youth of other nations were, this would not have come to pass. The public overlooked a good servant at its doors while waiting for the magic-working genii that were to come, but that are now farther off than ever, with the present result that the attitude of "wait till it develops" has become a chronic habit. Principal A. Jamieson, of the College of Science and Arts, Glasgow, referring to "Electrician's" letter, wrote:—"No one will dispute the fact that there does prevail a general lack of technical knowledge, more especially in connection with electric lighting, but this is not attributable to the science teaching in this country so much as to the apathetic and indifferent style in which young practical electricians as a rule seek to obtain that knowledge. Our foremost electricians are quite as well (if not better) versed in the theory of electricity and magnetism, and in its practical applications to telegraphy, telephony, electric lighting, and transmission of power as those of any other country. Our text-books and periodicals are quite as good, and the numerous Colleges of Science which have of late sprung up in almost every one of our large towns offer excellent opportunities for the study of the technology of many of our most important trades. It has always been a puzzle to me why the parents and guardians of young men who wish to learn any particular profession or trade wherein science and technical instruction are so necessary to a thorough training, do not send their boys to classes where such instruction may be obtained immediately after their leaving school or during their apprenticeship. It is a still greater puzzle why the employers do not, as a rule, inform parents that such instruction would benefit not only the young men but themselves. It is due to this in a large measure, no doubt, that the best students in our electrical engineering classes are seldom directly interested in the manufacture or care of electric lighting apparatus. There is not a more interesting study in the whole range of science than electricity and its applications, and the best students study these subjects partly owing to the enticing interest which they have, and partly for the marks which they may gain in competing for scholarships and prizes, while those who should do so in order that they may rise in their trade are comparatively few. . . . . Whenever the general dulness in trade has passed over and more prosperous times come round there can be no doubt that many will avail themselves of the luxury of electric lighting, and young men who have any special turn for the study of electrical engineering should now avail themselves of whatever science teaching there may be within their reach, in order that they may be ready to occupy positions of trust and remuneration when the tide turns."

In a subsequent letter, "Electrician" disclaimed any belief that our men of science—those engaged in the high researches of pure science or the practical-minded workers and inventors—were inferior to those of other nations. Neither did he wish to assert that the scholars

passing through such institutions as that over which Mr. Jamieson so energetically presides do not receive adequate training. On the contrary, he could bear witness to its utility, as quite a number of his apprentices and some of the younger workmen have attended Mr. Jamieson's lectures during a session or two and have been immensely the better for it.

The Compass and the Umbrella.—Messrs. Whyte and Co., the well-known scientific instrument makers of Glasgow, write to the *Glasgow Herald* in confirmation of Captain Campbell's story as to the derangement caused in the compass of the *Princess Beatrice* by a magnetised umbrella. Their letter follows, and is interesting:—"In your issue of the 21st, we noticed a paragraph about the influence of an umbrella on a compass on board the Larne steamer *Princess Beatrice*. In reference thereto we desire to say that the umbrella was submitted to us for the purpose of being tested, and we find the result to be such as Captain Campbell has already stated, the magnetism being as great as is found in an ordinary ten inch bar magnet. Any one acquainted with compass adjusting will be aware from this fact that it would deflect the compass as much as twenty degrees if held at the distance of only one foot from it, thereby endangering the vessel to no small extent. The manner in which umbrellas are usually held give it the character of an end on magnet, and our observations are based on that. When the umbrella was sent to us we looked at the matter as a joke, or something like that, but our testing has disabused our minds of that, and it now presents to us a very serious matter indeed. We would add as a warning to shipmasters and pilots that they see to it that passengers are not allowed to congregate round the navigating compass, otherwise the results may be any other thing than pleasant to all on board, and this being new danger to those who have their business on the water, a little attention now, on its discovery, may be the means of saving both valuable lives and property during fogs, which are so common on our coasts. Now, we can only express our fear that some wrecks of past years may have been caused by that simple and useful article of daily use, the umbrella. Hoping that as the matter has been brought before the public it may be productive of some necessary precautions to prevent accidents being made, we are, &c., WHYTE AND CO."

A correspondent of the same paper writes:—"I don't know whether you have an electric light machine on your premises or not, but if you have, just lay your umbrella—it does not matter whether it is a "liberal" one or not—beside it while it is running some night, and try the effect of it on a compass next morning. I guess you'll find that you can deflect the needle very much as you like. This, I know, has been my experience, and is, I think, a full explanation of the Larne and Stranraer phenomenon. Moral: Don't allow electric light folks to sail with Captain Campbell."

Electrical Catalogue.—About the finest specimen of an electrical catalogue we have seen is that which has just reached us from an Italian firm, Messrs. B. Cabella and Co. (Tecnomasio Italiano), of Milan. The illustrations are of large size, and are well executed, the paper and printing being also of good quality.

NEW COMPANY REGISTERED.

West African Telegraph Company, Limited.—Capital £1,000,000, in £10 shares. Object: To acquire telegraph wires and cables of all kinds for the purpose of telegraphs, telephones, or other purpose, and to carry on the business of telegraphy in all branches in all parts of the world. Signatories: A. Scott, 157, Leadenhall Street, 50 shares; J. Miles, 32, Paternoster Row, 100 shares; G. Henderson, 7, Mincing Lane, 50 shares; M. Gray, Abbey Wood, Kent, 100 shares; R. Kaye Gray, C.E., 106, Cannon Street; Neil Bannatyne, 15, Earl's Court Square, 50 shares; Wm. James Tyler, 106, Cannon Street, 25 shares. Directing qualification: Shares or stock of the nominal value of £500. The signatories are to appoint the first directors; remuneration, £2,000 per annum and £500 additional for each 1 per cent. dividend or bonus in excess of 6 per cent. per annum. Registered 20th ult., by Thomson & Sons, 57, Chancery Lane.

OFFICIAL RETURNS OF ELECTRICAL COMPANIES

Volta Electrical Company, Limited.—The first return of this company, made up to the 28th of August, was filed on the 24th ult. The nominal capital is £5,000 divided into 4,000 preference and 1,000 deferred shares of £1 each; 1,765 preference and 1,000 deferred shares have been taken up, and the full amount has been called and paid thereon. Registered office: 2, Chatham Buildings, Chatham Street, Manchester.

Universal Telephone Company, Limited.—The registered office of this company is situate at Dashwood House, New Broad Street, E.C.

CITY NOTES.

Eastern Extension, Australasia and China Telegraph Company, Limited.—The directors announce an interim dividend of 2s. 6d. per share for the quarter ended June 30th last, together with a bonus of 2s. per share, both free of income-tax, payable on the 15th proximo.

Globe Telegraph and Trust Company, Limited.—An interim dividend of 3s. per share, less income tax, on the preference shares, and of 1s. 6d. per share (income tax already deducted) on the ordinary shares, have been declared for the quarter to October 18th.

TRAFFIC RECEIPTS.

The Western and Brazilian Telegraph Company, Limited. The receipts for the week ending September 18th were £1,780; for the week ending September 25th, £2,201, after deducting the fifth of the gross receipts payable to the London Platino-Brazilian Telegraph Company, Limited.

NEW PATENTS—1885.

11300. "Dynamo-electric generators." T. J. HANDFORD. (Communicated by R. H. Mather.) Dated September 23. (*Complete.*)

11307. "Secondary voltaic batteries." C. MOSELEY, T. PARKER. Dated September 23.

11331. "Inducing and induced currents of secondary generators used for distribution of electrical energy." L. GAULARD, J. D. GIBBS. Dated September 23.

11340. "Dynamo-electric machines." F. H. VARLEY, VARLEY ELECTRIC PATENTS PROPRIETORY. Dated September 23.

11343. "Storage batteries." J. FRASER. Dated September 23.

11381. "Electric lamps." A. BRIN. Dated September 24.

11409. "Making carbon plates, tubes or cells for galvanic or secondary batteries." W. SYMONS. Dated September 25.

11421. "Filaments for electric glow lamps." J. SWINBURNE. Dated September 25.

11454. "Voltaic batteries." T. J. JONES. Dated September 25.

11500. "Telephonic instruments." S. P. THOMPSON. Dated September 26.

11501. "Telephones." S. P. THOMPSON. Dated September 26.

11510. "Mechanical telephones and lines for the same." R. H. RIDOUT. Dated September 28.

11511. "Earth and other clips employed in forming electrical circuits." A. WHALLEY. Dated September 28.

11513. "Appliances for jointing or connecting electric wires." A. WHALLEY. Dated September 28.

11566. "In-and-out electrical push indicator." N. G. THOMPSON. Dated September 29.

11568. "Electrotypes for printing purposes." A. F. WENGER. Dated September 29.

11569. "Laying electrical conductors in subterranean lines, and the construction of such lines." P. M. JUSTICE. (Communicated by H. C. Spalding and P. McMackin.) Dated September 29.

11570. "Electro-magnets." P. M. JUSTICE. (Communicated by H. C. Spalding.) Dated September 29.

11574. "Safety devices for electric circuits." P. M. JUSTICE. (Communicated by H. C. Spalding.) Dated September 29.

11608. "Telegraphic call instruments." A. K. PHILLIPS, A. W. GRANVILLE. (Communicated by F. Gridley.) Dated September 29.

11615. "Instruments for measuring and controlling electrical currents." J. H. DAVIES. Dated September 29.

11637. "Magnetic rotatory machine for separating iron and steel particles from brass filings, borings, &c." W. P. THOMPSON. (Communicated by H. Lamprecht.) Dated September 30.

11645. "Railway signalling by automatic electric lanterns." H. J. GARDNER. (Communicated by C. D. Tisdale.) Dated September 30.

11652. "Making plates or elements for voltaic batteries." T. J. JONES, W. H. TASKER. Dated September 30.

11659. "Battery jars." H. G. ELLERY, J. T. GENT. Dated September 30.

11571. "Electric cables, and apparatus for laying and securing same in position." P. M. JUSTICE. Dated September 29.

11572. "Operation of subterranean lines of electrical conductors." P. M. JUSTICE. (Communicated by H. C. Spalding.) Dated September 29.

11573. "Electrical cables" P. M. JUSTICE. (Communicated by H. C. SPALDING.) Dated September 29.

ABSTRACTS OF PUBLISHED SPECIFICATIONS, 1885.

6689. "Improvements in electric telephones." C. E. ALLEN. Dated June 2nd. 6d. The inventor employs, as part of the electric circuit, a body of conducting material in a solid, or in a loose free state, such as pulverised, powdered or granulated carbon. He places this material in a chamber with non-conducting walls and provides a platinum plate at bottom and another at top thereof. The bottom plate is fixed when in use, but is adjustable to increase or diminish the compactness of the free carbon, or to enlarge or lessen the capacity of the carbon chamber. The plate on the opposite end of the body of carbon is free to vibrate thereupon, and has a post for connection with the diaphragm. When the diaphragm is vibrated by the sound waves, the vibrating platinum plate is moved correspondingly against the free carbon. This causes the conductivity of the pulverised mass to vary, and a consequent variance of the working strength of the current passing through the instrument in agreement with the sound waves. The vibrating platinum plate or disc is held against the diaphragm by a delicately adjusted spring, so as to cause it to respond to the slightest vibrations of the diaphragm, and to quickly follow the outward movement thereof. The connection between the spring and the post, or part of the platinum plate, with which it makes contact is platinum covered, so as to make it sensitive to the electric contact, and, although the pressure of the spring against the post is continuous, yet this pressure varies in unison with the vibrations of the diaphragm and aids in varying the working strength of the current.

6994. "Improvements in smelting ores by an electric current, and in furnaces or apparatus therefor." W. P. THOMPSON. (Communicated from abroad by E. H. Cowles and A. H. Cowles, both of America.) Dated June 9. 6d. Consists of a furnace or apparatus in which the action of the current can be distributed over a large area or mass, and in the use, for metallurgical purposes, of a body of granular material of high resistance or low conductivity interposed within the circuit in such a manner as to form a continuous and unbroken part of the same, which granular body, by reason of its resistance, is made incandescent, and generates all the heat required. The ore or like material to be reduced, as, for example, the hydrated oxide of aluminium, alum, chloride of sodium, oxide of calcium, or sulphate of strontium, is usually mixed with the body of granular resistance material, and is thus brought directly in contact with the heat at the points of generation, at the same time the heat is distributed through the mass of granular material, being generated by the resistance of all the granules, and is not localised at one point or along a single line.

7321. "Improvements in carbonising filaments for electric lamps and in the apparatus connected therewith." C. A. DAY. (Communicated from abroad by F. Schaefer, of Bavaria.) Dated June 16. 6d. Filaments to be carbonised are wrapped or wound about a flexible or yielding form, preferably while the said form is held upon or supported internally by a mold shaped to correspond in cross section with the shape desired for the carbonised filament, the said form, the filaments having been wound thereon, being removed from the mold and then placed in a retort or crucible where the form and the filaments thereon are subjected to heat and carbonised, the said form within the filament carbonising and shrinking in unison with the filaments, thus obviating all strain upon the same, leaving the carbonised filaments unbroken upon the carbonised form, ready to be cut off as desired. In accordance with my invention, the form is composed of paper, cloth, or other material which will carbonise substantially in unison with or shrink to substantially the same degree as the thread filaments when the latter are being carbonised.

CORRESPONDENCE.

Life of Lamps at the Law Courts.

I have been considering Mr. Grundy's method, and though I fear it has no practical value, find it rather interesting theoretically. After a great lapse of time it would not necessarily be so ridiculous.

Supposing we start a large installation with lamps having any average life, say, 1,000 hours. Consider the whole time divided into hundreds of hours. All the lamps to run the same hours per day. Then for, say, the first 500 hours there will practically be a blank as regards breakages; then the number will gradually increase up to the 1,000th hour, and then gradually fall off, till the last lamp of the first set is gone; the cluster of breakages being thickest about the 1,000th hour. But the next blank will be less than 500 hours, owing to the longest-lived lamps lasting into the second period of 1,000 hours, and the first renewals going on the average earlier than any in the first period. Consequently the blank and the cluster will be less marked. This process would go on till, after a lapse of time compared with which the average lamp-life is small, the blanks and clusters would practically disappear, and the breakages would be evenly distributed over all the periods of 100 hours; or, indeed, any other period if the installation is large enough. When this point is reached, Mr. Grundy's method applies directly. You might take an arbitrary period, divide the total lamp hours of the installation by the breakages, and the quotient would give the average lamp life.

If this is a correct statement I do not think I could more completely exhibit the futility of Mr. Grundy's actual application of the method. He has taken a period, less than the average lamp life, early in the history of the installation. From his data the average lamp life is absolutely indeterminate. With good luck in the time selected for starting, the result arrived at might be accurately correct; with bad luck it might be 100 per cent. too much. From 40 to 50 per cent. is a very probable error.

Here is another amusing way of looking at his figures. The total installation has 705 lamps, of which 605 are in the courts and corridors. These latter, *i.e.*, six-sevenths of the installation, show by his method an average lamp life of over 3,000 hours, but the other seventh pulls it down to 1631·1. If only gas had been used in place of these 100 lamps, what a grand result would have been attained!

X.

30*th September*, 1885.

Lightning Conductors.

The letter of your correspondent on the above subject, in your issue of 19th September, very forcibly calls attention to the important subject of efficiently "earthing" lightning conductors.

In spite of the rule laid down by the Lightning Rod Conference, that the "earth-connection of a lightning conductor should be made by being attached to a copper plate not less than three feet square, well embedded in coke or carbon, and the result tested by a competent person," conductors are still being erected every day without any regard to this very necessary provision—some five or six feet of the conductor only being placed underground; and as for the testing—if it takes place—it is most frequently a delusion.

For testing earth-connections it is the common practice of even well-known firms to employ a simple detector galvanometer of the ordinary pattern used for testing electric bell wires, without even calibration, or a single coil, and so long as the needle be fairly deflected on completing the circuit with another "earth," are content to assume that the connection is perfectly satisfactory, being necessarily absolutely ignorant of its resistance, which might be 1 or 1000 ohms, for all anyone could tell by such a method of testing.

Not many months ago I was present at the so-called "testing" of a lightning conductor in the manner just

described. After testing for continuity with the detector, the gentleman who had the task in hand, asked what kind of an earth-connection had been made, and on being informed a copper plate had been well laid down, &c., was so good as to inform me that he *could not test the resistance of the earth-connection without digging the plate up.* On my naturally expressing surprise as to how such a wonderful feat was to be accomplished, I was further told that he had often dug up earth plates for the purpose of testing "connections." The detector was then brought into use to ascertain if the plate was connected to earth, and to obtain a corresponding earth to complete the circuit it was sought to obtain one off a newly painted rain water pipe, by roughly scraping off a small patch of paint with a penknife and then holding the bare end of the wire to the place so "cleaned." Scarcely any deflection was observed, as might have been expected, as rain water pipes do not always go to earth, but, as in this case, simply terminate above a glazed gully trap. On a water pipe being suggested a good deflection was obtained—and this was considered confirmatory of a satisfactory test I had previously taken with a very sensitive differential galvanometer, and set of coils specially designed for low measurements, and conveniently arranged for travelling.

It must, I think, be regarded as an unfortunate omission that the Lightning Rod Conference did not lay down what they regarded as the highest figure which should be allowed for the resistance of an earth-plate, and beyond which it would be dangerous to go. By all means have the resistance as low as possible in every case, and in the instance cited by your correspondent, with such favourable conditions close at hand, there could have been no excuse for the carelessness (may it not have been ignorance) which permitted such a resistance as he found. On the other hand, is it really necessary to strive after a resistance of "a quarter of an ohm?" Whether it be or not, one thing is certain, that such an approach to a perfect "earth" is in many places unattainable at any cost. For instance, take a building to be protected on the top of a hill—the hill and all the adjoining land for a mile or two round being composed, a few inches below the surface, of sand and pebbles—in fact almost entirely of small stones. Under such unfavourable circumstances a test taken between two plates at any reasonable distance apart would reveal a resistance of many units. Even in light sand, almost free from stones, where the bed of sand has been of considerable depth, I have known the resistance between two copper plates (forty-six yards apart), as recommended by the Lightning Rod Conference, and laid in a carefully prepared bed of crushed coke eight feet square, to be as great as sixteen ohms. The resistance between two main water pipes fifty feet apart in the same grounds was under half an ohm. On testing each plate separately from one of the main water pipes, almost equidistant between the two plates, one gave 7 ohms and the other 8½ ohms. This result simply confirmed what is well known, that the question of a good "earth" is simply one of area of connection, except in rock or stones, &c. The area of the copper plates being infinitely small in comparison with that of the extended underground pipes used in substitution of a test plate.

Considering the high tension of lightning and its power of overcoming resistance, can it be confidently affirmed that a lightning conductor (good in all other respects) had ever failed to do its duty, even with an earth resistance of 20 ohms? This was an outside limit given me by an eminent electrician (the late Mr. Robert Sabine) for bad earths, but within which he considered no danger would arise. During the last few years I have on four or five occasions, when testing old lightning conductors, been credibly informed by the persons in charge of the buildings that there was no need to test the conductor, as they "had seen the lightning run down it." Yet in no instance was the resistance so low as "a quarter of an ohm"—the lowest being 8 ohms and the highest 17. In none of these instances had either the building or the conductor sustained any damage, and they forcibly reminded me at the time of the 20 ohms limit above assigned.

There are several other points on the question of testing earth connections which naturally occur to any one conversant with the subject, but upon which the Lightning Rod Conference Rules are silent. Fearing, however, this communication may already prove too lengthy for your valuable space, I reserve them for a future occasion, should you consider them of sufficient interest.

Geo. Driver.

29*th September*, 1885.

Compound-Wound Dynamos.

I notice in your last issue a communication calling in question the validity of the patent belonging to the Anglo-American Brush Electric Light Corporation under which they claim the form of machine commonly known as the "Compound-Wound Self-Regulating Dynamo."

The ground given by your correspondent for arriving at his conclusion is that the said patent has been anticipated by one of earlier date, from which he quotes a short extract.

The mare's nest discovered and thus put forward with so much mystery, consists of a few lines selected from a patent of many folios granted to Mr. S. A. Varley in 1876 (No. 4905) about two years in advance of Mr. Brush's specification.

I can only suppose that your correspondent has not read the context, otherwise I would suggest that before committing himself to so definite an opinion, it might have been better if he had secured the assistance of an electrician who could possibly have made it clear to him that the description referred to has *nothing whatever to do* with the point at issue. When properly understood, I venture to think that it would overtax even his ingenuity to distort Mr. Varley's idea into a form to justify his statement. And Mr. Varley himself would, I am sure, be the first to stand aghast at any expertism which utterly disregards the most elementary principles of cause and effect.

Until your correspondent can rake up some better grounds for attack I fear he must be content to submit to the fact—which seems to disturb his peace of mind—that the rights claimed by the Brush Company are justly their property.

John S. Sellon.

Sydenham Hall,
30*th September*, 1885.

With regard to Mr. Beeman's article on compound winding, I would like to point out that Mr. Brush did not even invent what is now exclusively known as compound winding, that is, double winding the field magnets so as to give constant E.M.F. on a varying number of lamps.

Though Mr. Varley mentions both series and shunt wires, the arrangement is merely to keep the magnets excited when the main circuit is opened. And neither his patent nor Mr. Brush's mentions winding for constant E.M.F. Dr. Higgs's American patent, 242,644, is for the same thing, and Dr. Higgs specially mentions that he wants the wires wound on different field-magnets altogether. He also states later that he did not succeed in making machines work with double-wound field magnets. (*Electrician*, December 23rd, 1883.

As I pointed out in the same number of the *Electrician*, the first compound dynamos for constant E.M.F. were made by the Brush Company of Cleveland for me; and in my conversation with Mr. Brush when ordering the machines, he advised me not to have compound dynamos, as he had never tried them, and did not think they would work. He had heard of the idea through Mr. Edmunds, who knew me in Paris, and had gone to America before me. Mr. Brush, in his letter to me giving the figures of the trials of the dynamos, expresses his astonishment at the results.

The first patent mentioning compound winding, in the present sense of the term, is Edison's in America, No.

264,688. And the first in England is Mr. F. C. Phillips's, 4691, 1882; but he, unfortunately, did not claim it, thinking it old. Mr. Bosanquet ascribed the invention to Mr. Swan, but Mr. Swan did not claim it, as he of course got it from me. I was manager of his factory in Paris, and I had asked him to patent it for me about a year before. I applied for a patent in America, not only for the compound winding, but for the method of winding referred to in my letter to the *Electrician*, above-mentioned. This was for obviating the necessity of moving the brushes with a varying load. With this object the coils were wound end to end, and put on unsymmetrically, so that the armature ran from a shunt coil to a series coil. The magnets themselves might be uneven, so that the brushes should always be at right angles to the magnetic centre line, instead of being put forward. I am abandoning this on account of the expense of interference, not with Mr. Brush but with Mr. Edison, though my dynamos were running when his patent was taken out.

Jas. Swinburne.

The Patents (Amendment) Act, 1885.

Perhaps the following notice of The Patents Designs and Trade Marks (Amendment) Act, 1885, passed towards the close of the last Session of Parliament may be found acceptable to your readers.

This Act, although numbering only six sections, embraces some very important provisions, amongst which may be mentioned the following, viz. :—

Power is given to the Comptroller to dispense with the statutory declaration hitherto required at the time of lodging the application for a patent, and also to extend by one month the time of filing the complete specification, and by three months the time within which the complete specification must be accepted. The rules framed in compliance with the Act are published in the numbers of the Official Journal of the Patent Office for September 8th and September 11th, and are now in force.

As a result of these changes in the law and rules, applicants for patents will simply have to sign the application form duly filled up, the attestation of a commissioner, justice or other functionary being no longer required; and on showing good grounds and paying moderate fees, they may obtain the extensions of time provided for in the Act.

Further, "where an application for a patent has been abandoned or become void, the specification or specifications and drawings (if any) accompanying or left in connection with such application, shall not at any time be open to public inspection or be published by the comptroller."—*Section* 4.

In pursuance of this enactment instructions have been received at the inspection department of the Patent Office not to allow lapsed provisional specifications to be inspected in future; and, as they are no longer printed, abandoned specifications will not stand in the way of subsequent patentees.

Thos. J. Handford.

50, Bedford Row, W.C.,
September 26*th*.

Reis's Telephones.

I have been greatly interested in reading the article reproduced in your issue of 5th September, under the heading of the "Reis Telephones," and the correspondence which has since taken place, and should like, with your permission, to add a few remarks before the subject is dismissed. My object, however, in now addressing you is not so much to enter into the broad and vexed question as to how far Reis's telephones constitute an anticipation of Bell's claims, but rather to offer a few observations on that portion of the article to which you in an editorial note called special attention.

The writer of the article in the *Scientific American* says: "It is a well received principle in patent law that an invention is protected for all possible uses, even for such as were unforeseen by the inventor."

Now this statement may be to some extent true in respect of American law, but as applied to patent law in this country the doctrine it involves is at any rate altogether new to me. The law, in my judgment, is that the invention is only protected for the uses covered by the patentee in his specification, not necessarily all described, but comprised and included in the language of his title and claims.

As, however, an example is frequently more conclusive than a good deal of abstract reasoning, I will assume a simple case :—

A has invented a machine adapted for grinding corn or other grain and takes out a patent for it as a "Machine for grinding grain;" B discovers that a similar machine will answer for grinding mineral substances and uses it for that purpose. If I am right, this user is not an infringement of the patent for the corn grinding machine. At the same time it is extremely doubtful whether B could sustain a patent for this application of A's machine, which A should have covered in his own patent but, omitting by oversight, unintentionally makes a present of to the British public.

It will be seen that my views on this subject are in agreement with those put forth by Professor Silvanus Thompson in a letter published in the REVIEW for 7th June, 1884, in reference to a letter written by me on telephone patents. I cannot find any response of mine to that gentleman's letter, but I take this opportunity of saying that I intended my statement to be taken in the limited sense which he kindly pointed out; and had I thought that it would have been open to misconstruction, should have certainly mentioned the qualification alluded to.

I ought, however, to add (following out the case I have assumed above) that the Courts would look with a very jealous eye on machines manufactured for export, ostensibly for grinding mineral substances, to see that A's interests were duly guarded, just as in case of a war between two foreign powers with which we are at peace, vessels and munition of war built and produced here and purporting to be bound for a neutral State, are closely watched and frequently seized, on the ground that they are really intended for one of the belligerents.

Thos. J. Handford.

50, Bedford Row, W.C.,
September 28*th*.

[With reference to the foregoing letter we would refer Mr. Handford to the case of "Clark *v.* Adie," in Law Reports, Vol. II. of the Appeal Cases, pages 320-321 and the middle of page 328; see also the report of the case in the Court of Appeal, Law Reports 10 Chancery Appeals, last line of page 676.—EDS. ELEC. REV.]

I did not intend to argue the question of Reis *v.* Bell with Prof. Silvanus P. Thompson. It simply occurred to me when reading his version of "Tonsprache" that he was basing his argument upon a "fallacy," and hence took the liberty of directing his and other electricians' attention to it. But even the task he sets me of translating for him "How a single instrument should reproduce at the same time all the effects of the organs which are active with human speech? That was always the cardinal question," and which he gives in his letter pretty correctly, does not state that Reis *had done* it.

I may mention, by-the-bye, that I am old enough to remember very well Dr. Reis's instrument being spoken of; but, I must confess, I did not hear at the time that it could *talk*. It could "Lante von sich gebend," reproduce laughter, coughing, whistling, and other noises. Prof. Thompson makes Reis say himself: "Until now it has not been possible to reproduce the tones of human speech, &c."

Why try to prove that he did by faulty translation, or declaring a horse chestnut to be a chestnut horse?

G. R.

September 28*th*, 1885.

P.S.—Thanks for your correcting the "cat."